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THE COMPOUND AND MIXED NESTS OF AMERICAN ANTS.

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PART III. SYMBIOGENESIS AND PSYCHOGENESIS.

"Eine Psychologie in Spencer-Darwin'schem Sinne auf Entwicklungslehre gegründet, aber auf positiver Detailforschung fussend, verspricht reichere Resultate als alle bisherigen Speculationen." — E. MACH.

ALL writers on the behavior of ants have been deeply impressed with the cases of social symbiosis, more especially with those of an extreme type like Polyergus and Anergates. The deadly animosity of the members of a formicary, not only towards ants of another species but even towards ants of the same species belonging to different colonies, is so striking and so nearly universal that an extraordinary explanation seems to be demanded to account for the cases of amicable consociation of two species. In the presence of such phenomena, instinct and consentience, or sense-experience, have been abandoned as inadequate, and the existence in ants of some higher form of intelligence, like understanding and ratiocination, has been postulated without further ado. At the same time the evolutionist has been stimulated to broach a phylogeny of social

symbiosis. Both the stress laid on the psychical manifestations of ants and the attempts at establishing a phylogeny of the compound and mixed nests have been clearly apprehended and set forth in considerable detail by Wasmann ('91). While my own observations lead me to agree with this able investigator in many respects, I must, nevertheless, dissent from his attitude towards the genetic method as applied to the study of the compound and mixed nests. It is necessary, therefore, to attempt a critical revision of this matter so far as this is possible within the limits of the present paper. I shall deal first with phylogeny as applied to the cases of social symbiosis and conclude with a very brief consideration of some of the pertinent psychical problems.

Darwin was the first to attempt an explanation of the origin of dulosis in the European ants. In a well-known passage in the "Origin" ('61, p. 244) he says: "By what steps the instinct of *Formica sanguinea* originated I will not pretend to conjecture. But as ants, which are not slave-makers, will, as I have seen, carry off pupæ of other species, if scattered near their nests, it is possible that such pupæ originally stored as food might become developed; and the foreign ants thus unintentionally reared would then follow their proper instincts, and do what work they could. If their presence proved useful to the species which had seized them — if it were more advantageous to this species to capture workers than to procreate them — the habit of collecting pupæ originally for food might by natural selection be strengthened and rendered permanent for the very different purpose of raising slaves. When the instinct was once acquired, if carried out to a much less extent even than in our British *F. sanguinea*, which, as we have seen, is less aided by its slaves than the same species in Switzerland, natural selection might increase and modify the instinct — always supposing each modification to be of use to the species — until an ant was formed as abjectly dependent on its slaves as is the *Formica rufescens*."

Apart from the statement that the English and Swiss *sanguinea* differ in their behavior — a statement which has been since disproved — Darwin's views have been accepted by Forel

('74, p. 440), who has also called attention to the fact that the frequent occurrence of pupæ without cocoons in the nests of *F. fusca* would add to the plausibility of Darwin's hypothesis, for such free pupæ would be able to hatch without the assistance of the enslaving species.[1]

Pursuing Darwin's line of thought, Forel ('74, p. 443) called attention to the following series, which seems to have been previously in great part suggested by von Hagens ('67): "1. Working ants pure and simple; 2. ants dwelling in abnormal mixed colonies; 3. *F. sanguinea* (sometimes without slaves); 4. *Polyergus rufescens* (here the working instinct, which is merely diminished in *F. sanguinea*, disappears completely and the slave-making instinct attains its apogee); 5. *Strongylognathus huberi* (the slave-making instinct is certainly still alive); 6. *S. testaceus* (the slave-making instinct no longer exists except in the form of derisory vestiges, the worker is on the road to atrophy and tends to disappear); 7. *Anergates atratulus* (the worker has disappeared; only parasitism is admissible). This last ant, it seems to me, is a remarkable example of reversion to ancestral traits (incomplete societies, without workers) through parasitism; its genealogy is explicable through *S. testaceus*, the workers of which have become so rare in comparison with the females and males." Essentially the same series of cases was adopted by Lubbock ('94, pp. 88, 89). At the present time it could be still further perfected, as Wasmann suggests, by the insertion of *Tomognathus sublævis* between *F. sanguinea* and Polyergus.

The views initiated by Darwin have not been allowed to pass unchallenged. The first to take up the cudgels was McCook in the concluding paragraphs of his paper on *Polyergus lucidus* ('80, pp. 383, 384). After presenting Darwin's views he writes: "Whatever credit we may give to this ingenious hypothesis, it must be said that in the case of our *F. schaufussi*, natural selection has not operated to degenerate the soldierly courage and faculty, and remand the duty of defense to those associates in whom the military faculty

[1] The American slaves of *F. sanguinea* and Polyergus, *viz.*, *F. fusca*, vars., *subsericea* and *subænescens*, and *F. nitidiventris* also often have free pupæ.

has been specialized. In other words, if Lucidus has become specialized as a warrior, dropping an original disposition and ability to labor, her slave has not become specialized as a worker, nor dropped her combative faculty, but seems to be possessed in all respects of the normal habits and nature of ants of her species. At least I could trace in her no effects of slavery, other than the strange association with and care of her abductor. One, therefore, who accepts Dr. Darwin's suggestion, must allow that natural selection has wrought toward specialization in one section of the colony, but has been suspended in its operations upon the other section. It is doubtful if the anomalous conditions thus raised by Dr. Darwin's explanation be not more difficult to explain than the original conditions to which the hypothesis was applied."

Assuredly, if all the arguments against natural selection were as easily refuted as the one here produced by McCook, Darwin's followers would have occasion for great rejoicing. The refutation is equally easy whether we consider the colonies of the auxiliary species with which Polyergus wars, or only the larvæ and pupæ which it abducts from these colonies and rears as its slaves. So far as the latter are concerned, McCook has completely overlooked the very obvious fact that Polyergus rears only the workers of the auxiliary species, and these have never been known to reproduce in the mixed nests. But even if we suppose, for reasons to be given below, that these workers may often reproduce normally, the difficulty is not removed, since they would not only have to be able to transmit characters acquired in their imaginal stage, — and the possibility of this has by no means been demonstrated, — but they would have to transmit these characters to male or female offspring if there was to be any permanent modification of the species. Now this is impossible, because Polyergus will not permit the winged sexes of the auxiliary species to reach maturity. Hence the detached auxiliaries cannot impart any modifications to the species to which they belong, no matter what peculiarities they may take on in the Polyergus nests.

Though the fallacy in McCook's argument is very obvious, Wasmann still contends that it involves "einen vorzüglichen

Gedanken," which he puts in the form of the following question: "If natural selection can bring about such a peculiar development of the instincts in the dominant species, why has it not exercised a corresponding influence over the enslaved species?" Wasmann is evidently bent on rescuing that portion of the argument not expressly stated by McCook, *viz.*, the necessity of a change of character in the ants of the colonies attacked by Polyergus. But closer inspection shows this attempt to be unsuccessful, for he has not considered the question of the relative abundance of the dominant and auxiliary species. That this is a matter of some moment in a discussion of this kind is seen from a general survey of parasitic and predaceous organisms. These must of necessity live on that margin of surplus vitality and reproductivity so characteristic of all animals and plants; for it is obvious that organisms which depend very decidedly for their sustenance on special hosts or prey must endanger their own existence to the extent that they endanger the existence of the species on which they depend. The serious injury or death of the host species implies the death of the parasitic species in all cases where the relations between the two are of a highly specialized character. This argument could be adduced if the Polyergus were more abundant or quite as abundant as the auxiliary species. But it is quite unnecessary to make use of it, because Polyergus is a rare ant of local occurrence, and the various forms of *F. fusca* and *F. pallide-fulva* which it enslaves are widely distributed and vastly more abundant both in colonies and individuals. In fact, no other insects are as common in our Northern States as the varieties of these two ants, and even in Texas, near the southernmost limits of the distribution of *F. fusca*, a form very closely related to *F. subsericea*, *viz.*, *F. gnava* Buckley, is the most prolific of ants. Its nests often contain upwards of thirty fertile queens, and the number of eggs, larvæ, and pupæ which are reared between the end of February and the first of June is enormous. Why, then, should these very prolific and widely distributed species exhibit a special development of valor or a particular defensive form of nest architecture in adaptation to a rare predaceous

ant of sporadic occurrence? As well might we expect the human dermis and its appendages to present hereditary modifications in adaptation to occasional parasites like *Pediculus capitis*. The advantages that would result from the development of a courageous disposition and specially protective forms of nest architecture in *F. fusca* and *pallide-fulva* are more than outweighed by those derived from their unusual powers of reproduction. These species run to offspring, not to valor. That the survival and even the predominance of species does not necessarily depend on the development of moral and psychic endowment is demonstrated on a grand scale in the vegetable kingdom.

This defense of McCook's argument is, however, only a small portion of Wasmann's criticism of the position held by Darwin, Forel, and Lubbock in regard to the phylogeny of the mixed colonies ('**91**, pp. 214–254). Wasmann sees in these same facts irrefutable arguments against the theory of indeterminate variation and natural selection, and arguments equally strong in favor of tracing the modicum of physical and psychical development which is acceptable to him to "innere gesetzmässig wirkende Entwicklungsursachen." In my opinion, he has succeeded — if indeed he has really succeeded — only in showing that the genetic method has been somewhat awkwardly applied to the cases of compound and mixed nests. On the whole, I believe that he has neither invalidated the principle of natural selection, nor made it perfectly clear that we must forthwith deliver ourselves up to anything so impalpable as "innere gesetzmässig wirkende Entwicklungsursachen." In support of these statements the following remarks on some of Wasmann's arguments are offered.

Wasmann seems to regard it as an established fact that worker ants do not reproduce, or do so only under unusual or even pathological conditions. He is also inclined to emphasize the differences between the instincts of the queens and those of the workers. Hence the workers are debarred from transmitting their peculiar characters, either congenital or acquired, and the instinct modifications, so characteristic of different species of ants, must be explained as arising from determinate,

internally regulated variations. The premises to these conclusions I cannot accept, since they do not appear to me to be indefectibly established. They are based on a rather limited study of a few species of highly specialized European ants, and cannot, therefore, lay claim to great generality. My own observations, still incomplete, to be sure, on several Texan ants representing both the most primitive and the most specialized subfamilies (Ponerinæ and Camponotinæ) convince me that worker ants not only very frequently lay eggs in considerable numbers, but that these produce perfectly normal offspring. When workers are properly fed in the artificial nests, they seem to have no more desire to devour their own eggs than to devour those which are deposited in their keeping by the queens. During February last a carefully isolated lot of workers and soldiers of a handsome Camponotus (*C. texanus* n. sp.) laid dozens of eggs in my Fielde nests. By the first week in June many of the larvæ were mature, and a few of these had spun their cocoons before I was compelled to leave my laboratory for the summer. These cocoons were found to contain perfectly normal male pupæ, thus adding fresh evidence for the generally accepted belief that the parthenogenetic offspring of worker ants are males. Similar observations were made on workers of *Camponotus marginatus* n. var. and Pachycondyla, except that in these cases I did not follow the larvæ quite to the pupal stage. I am, moreover, convinced that numerous eggs are laid (probably by the soldiers) and reared in the frequently queenless nests of a gall-inhabiting Colobopsis (*C. etiolata* n. sp.) from Texas. In fact, for aught we know to the contrary, every well-developed ant colony may contain one or more fertile workers. Where the worker caste is dimorphic the soldiers probably have the greatest tendency to lay eggs. Judging by analogy with other Hymenoptera, like Polistes among the wasps, it is also probable that the older and more vigorous the ant colony, the greater the tendency for workers to take on the reproductive powers of the queen. That these conditions clearly imply the possibility of the inheritance of worker characters through the male offspring goes without saying. The comparatively frequent development of

the reproductive power in worker ants makes it possible to account for the conditions presented by Leptogenys and Tomognathus. In these genera worker forms (ergatoids) have usurped the functions of the winged queens, which have completely disappeared.[1]

The statements of Wasmann and other authors concerning the differences between the instincts of the queens and workers seem to me to require some qualification. This difference is rather quantitative than qualitative, for the recently fertilized queen, even in highly specialized ants, during the establishment of her colony displays nearly all the worker instincts, even to excavating and defending the nest and caring for the first brood of young. In some species (Ponerinæ ?) she may even exhibit the foraging instinct so characteristic of the workers, for aught we know to the contrary. It is true that in the more highly specialized ants like Formica, these instincts lapse into desuetude as soon as the workers make their appearance in the nest, but it is equally true that they may be retained throughout life as in the queens of the Ponerinæ, Leptothorax, and probably also many other ants. While I do not wish to lay unwonted stress on these fragmentary observations and reflections, they are, nevertheless, quite sufficient to bid us hesitate in the use of arguments which start from the assumption that the worker ants reproduce only under pathological conditions and present instincts essentially different from those of the queens.

Wasmann encounters the gravest difficulties in the genetic explanation of dulosis. His remarks are mainly confined to the two well-known cases, *Formica sanguinea* and Polyergus, the former, in the opinion of most writers, an incipiently, the latter a perfectly, dulotic species. He attempts to show that dulosis could not have arisen in *sanguinea* by selection, since flourishing or medium-sized colonies of this species could have derived no advantage from the possession of a small number of slaves, while the advantage that would accrue to a small colony

[1] Silvestri ('01), in a paper received while these paragraphs are going through the press, expresses some very similar views concerning the fecundity of worker Termites.

would be more than outweighed by the difficulty such a colony would experience in obtaining slaves.[1] And even if the dulotic habit had manifested itself repeatedly as a chance variation, and had proved useful, there would still be lacking the hereditary basis for the instinct, since this is exhibited only by the workers. Wasmann states the difficulty in the following words ('91 p. 236): "Before the inclination to rear slaves had proved itself permanently beneficial during many generations, it could not have been established by natural selection as an hereditary Anlage; but the possibility of inheriting the Anlage must exist before the incipient inclination could be transmitted from one colony to another — *ergo* natural selection lacks the very point of departure for the development of an hereditary slave-making instinct from the accidental forms of mixed colonies. We must leave natural selection like Baron von Münchhausen to drag itself out of the morass by its own hair."

Surely natural selection, however numerous its shortcomings, deserves better treatment at our hands. A careful perusal, however, of the above-quoted passage from the "Origin" and a consideration of the facts brought to light since its publication leave little cause for anxiety. Wasmann assumes that *sanguinea* robs the pupæ of other ants *for the sake* of rearing slaves. This is scarcely borne out by the facts. The young of the auxiliary species are sought for and appropriated to serve as food, in obedience to an ancient and widespread formicid instinct that emerges to view very clearly in many often distantly related species of ants. Thus Adlerz has shown ('96a) that on exceptional occasions even *Lasius niger* robs the larvæ and pupæ of *L. flavus*, and these may hatch and function as slaves in the nest of the dark-colored species. Wasmann, too ('99a), has observed a colony of *L. fuliginosus* appropriating the larvæ and pupæ of a neighboring colony of *Myrmica lævinodis*. I have repeatedly observed the same instinct in Mexican and Texan Ecitons ('01; *E. crassicorne, schmitti, pilosum*).[2] It is also probable that many cases

[1] This latter statement is disproved, at least so far as the American *sanguinea* is concerned, by the above-quoted observation of Forel.

[2] Although these ants often store the larvæ and pupæ in their nests for some time, they are probably never allowed to develop; but even if some of them

of synclerobiosis (especially those of *F. exsectoides* with *subsericea*) are due to a sporadic outcropping of this ancient instinct.[1] In view of the further facts that *sanguinea* can get on perfectly without auxiliaries, that it is the young colonies which usually contain the greatest number of slaves, and that the number of these is often highly variable in different colonies even in the same localities, we are certainly justified in demanding more stringent proof that *sanguinea* really robs for the sake of rearing slaves.[2] Laying most stress on the fact that the booty serves as food, — and of this Forel's observations contain sufficient evidence ('**74**, p. 258),[3] — we may regard the imaginal auxiliaries in the *sanguinea* nests as a *mere by-product*, as it were, of the colonial activities. The *sanguinea* often appropriate more food than they can devour, and the residuum merely adds workers to the colony, which are not harmful and may even be advantageous. This is evidently the interpretation intended by Darwin, who does not pretend to invoke the principle of natural selection in his genetic explanation of the *sanguinea* stage of dulosis, so that Wasmann has taken unnecessary pains to refute an imaginary argument. With

should hatch, the nomadic habits of the Ecitons and their poorly developed deportation instinct would prevent the formation of permanent mixed colonies, since the larvæ and pupæ which they kidnap belong to home-loving species.

[1] At Colebrook, Conn., I recently found three cases of synclerobiosis in addition to those enumerated in the second part of this paper: (1) a colony of *F. exsectoides* with *F. subsericea*, similar to the mixed colonies of these species observed by Forel and Schmitt; (2) *F. nitidiventris* with *F. rufa* var. *obscuripes* Forel; (3) *F. nitidiventris* with *F. rufa* var. *difficilis* Emery. All of these colonies were small, and in none of them could I find the queens of either of the consociating species.

[2] Wasmann believes ('**91**, p. 198) that in *sanguinea* the perception of the pupæ and the idea of their abduction are intimately connected by association with the idea of the auxiliaries to be bred from them, and he concludes: "Hieraus dürfte sich die Neigung der *sanguinea* zum Sklavenraub und zur Aufzucht fremder Hilfsameisen am leichtesten begreifen." This is one of the cases in which I am inclined to believe that Wasmann has greatly overestimated the power of association in ants. His statement, however, as he himself would probably admit, can hardly be regarded as proof of the point under discussion.

[3] "The *F. sanguinea* rarely rear all the cocoons of *F. pratensis* which they are given. Of these they often devour a great portion. One *sanguinea* formicary to which I gave a fabulous number of *F. pratensis* cocoons during the course of the summer failed to rear a single one. The same was true of several other formicaries to which I gave fewer cocoons."

F. sanguinea, however, there is already given the generalized condition required as a starting point for the action of natural selection and the development under its guidance of cases like Polyergus, as Darwin suggests. In Polyergus the predatory instincts have been developed to a highly specialized condition, while the domestic instincts have retrograded *pari passu* as a natural result of the survival of the prey, till the presence of slaves in the nest has become a *conditio sine quâ non* of existence. This correlation of instincts has involved the corresponding correlation of structure which we find so beautifully exhibited in Polyergus.[1] In the predatory instincts every slight variation in advance would be beneficial to the species, while slight retrogressions would not under the circumstances be disadvantageous. I cannot, therefore, agree with Wasmann when he says ('91, p. 247): "Natural selection could only maintain and augment useful instinct variations: but the development of slavery up to the Polyergus-, Strongylognathus-, and Anergates-stage is beneficial neither to the masters nor the slaves — *ergo* natural selection cannot have produced the instincts of the slave-holding ants." As good an answer as I can conceive to an argument of this nature is a reference to the cases of extremely specialized parasitism like the Cestode and Sacculina, both of which are connected by tolerably complete series of intermediate forms with the more generalized, non-parasitic members of their respective phyla.

The symbiotic sequence suggested by Forel and Lubbock is objected to by Wasmann on fairly good grounds. It is by no means clear that the development has passed successively through the stages represented by these forms. Indeed, as Wasmann shows, the problem of symbiogenesis is much more complicated than it appeared when the above sequence was suggested. It now seems evident that several lines of development have proceeded independently from cases of plesiobiosis (and possibly also parabiosis), which constituted the necessary initial stages of symbiogenesis. Thus it is probable that cleptobiosis, xenobiosis, and dulosis represent at least three

[1] In accounting for this development of instincts and structures, it is, of course, necessary to regard the whole mixed colony as a single evolutionary unit.

discrete lines of development, the two former starting from conditions of plesiobiosis, the latter from the widespread instincts of ants to prey on the offspring of other Formicidæ. Other cases which obviously resemble true dulosis may have arisen from xenobiosis. This appears to be true of cases like *Leptothorax emersoni* and possibly also of the species of Strongylognathus and Tomognathus. On the other hand, the cases of colacobiosis may be conceived to have originated either from xenobiotic conditions like that of *L. emersoni* or from dulotic conditions like that of *Strongylognathus testaceus*. I cannot believe that Forel or Lubbock really intended their sequence as anything more than a rather general attempt in concrete language to account for the phylogenetic derivation of the remarkable cases of social parasitism (Anergates) from the simpler forms of mixed nests. It is therefore superfluous to waste many words for the sake of showing that the ants of the Forel-Lubbock series are not phylogenetically related. It is not only easier to sketch the phylogeny of the compound and mixed nests in bold outlines than to fill in the details, as Wasmann somewhat reproachfully suggests, but this is the only available method of procedure at the present time. Still even the attempt at detailed speculation in this direction scarcely merits our disapproval as it does Wasmann's, for free and open speculation is necessary to the advancement of a scientific subject, if only as furnishing the necessary incentives and guides to the attainment of profounder insight. Mere fact-culling is not and never can be science.

Another argument on which Wasmann lays some stress is drawn from the supposed immutability of instinct.[1] The instincts of *F. sanguinea* and Polyergus are regarded as identical both in Europe and America, and these instincts must therefore have remained unchanged for a very long period of time ('91, p. 249). "Huber's amazons of 1804 fought and

[1] It is unnecessary in this place to deal with the doctrine of the immutability of instinct so brilliantly advocated by Fabre ('79–'00). That it is quite untenable has been demonstrated by Dr. and Mrs. Peckham ('98), Whitman ('99), and others. It could, in fact, be demonstrated to be false from Fabre's own magnificent observations. The genus Leptothorax, considered below, furnishes additional evidence, if this were needed.

conquered exactly like Forel's amazons of 1870, and proved themselves to be quite as dependent on their slaves as their modern descendants; and there can be no doubt, that if Adam had studied and described the habits of the amazons, his account would agree very accurately with Huber's and Forel's." Similarly, Wasmann stresses the long-existing fixity of instinct in Formicoxenus and Leptothorax (p. 226). No issue can be taken with him on this point — but what phylogenist would not take it for granted? If structure can remain stable during æons of geological time, certainly instinct may also remain relatively unchanged. It is, however, equally true — and this point seems not to have been considered by Wasmann — that structure may undergo little change as compared with instinct. In support of this statement I would include in this place a series of facts which may have arrested the attention of the reader in the previous portions of this paper, *viz.*, *the remarkable differences of instinct exhibited by the species of the single genus Leptothorax.* Morphologically, this very large and widely distributed genus has been justly styled "homogeneous" by Forel ('74, p. 339), since the numerous species are closely related to one another and often separable only on rather trivial characters. Even the subgenera Dichothorax and Temnothorax are based on relatively slight differences. In their habits, on the other hand, the species of Leptothorax are singularly diverse. Many of the forms have no tendency to consort with ants of other species, but differ considerably in the stations which they inhabit. Some prefer to live under stones, others in moss, others under bark or in dead wood, and still others, like one of the Texan species, in cynipid galls, or, like our New England *L. longispinosus* Rog., in worm-eaten hickory nuts among the dead leaves under the trees. Many species, however, have a pronounced *penchant* for entering into more or less intimate symbiotic relations with other Formicidæ, as shown in the following conspectus:

1. The European *L. muscorum* often lives in plesiobiosis with *Formica rufa* (see pp. 519, 520).

2. A similar tendency is undoubtedly exhibited by our

American *L. canadensis* Provencher,[1] which I have had occasion to observe since the second part of this paper was written. June 21, I found at Cudahy, near Milwaukee, Wis., two nests of this Leptothorax compounded with a large nest of *Cremastogaster lineolata* Say in an old oak stump. The galleries of the Cremastogaster extended far into the dry rotten wood and contained numerous worker larvæ and pupæ. The two Leptothorax nests, which together scarcely contained more than sixty to seventy ants, were about twenty inches apart. Each was a simple chamber one-half to three-quarters of an inch in diameter excavated in the thick bark, and each communicated by means of a slender passage with the subcortical space, which was used as a common runway by the workers of both species. When the bark was stripped off, several of the Leptothorax were seen running on the surface of the wood and mingling with the Cremastogasters, which they closely resembled in coloration and sculpture, though differing in size, shape, and movement. The Leptothorax were very timid, and when touched with the fingers or tweezers, at once curled up and "feigned death," after the manner of Myrmecina. One of the nests contained several mature larvæ and pseudonymphs. These the workers and the single deälated queen were hastily removing to a place of safety. Rain began to fall soon after I had opened this interesting compound nest, and I was obliged to continue my observations on some living specimens of both the species hurriedly confined to a small vial. When the two species met, as they often did in these narrow quarters, the Cremastogaster stroked the Leptothorax with its antennæ. On such occasions the latter at once crouched motionless and folded its antennæ along the sides of its head. Then sometimes the Cremastogaster would stand over the little ant and lick its rugose head and thorax; at other times it would pass on without bestowing these attentions on the Leptothorax, which at once sprang to its feet and ran away. This performance was repeated so often during the remainder of the day that there could be no doubt

[1] According to Emery ('94, p. 318) this ant is perhaps only a subspecies of the European *L. acervorum*.

concerning the friendly relations of the two species. By the following morning the Leptothorax and most of the Cremastogasters were either dead or dying, having been suffocated by the pungent exhalations of the latter species. Thus the little I could observe of the relations of the two species resembled those which I have recorded for *L. emersoni* and *Myrmica brevinodis* — only reversed, the *L. canadensis* behaving like the Myrmica, while the Cremastogaster behaved somewhat like *L. emersoni*.[1]

3. *L. pergandei* lives, probably as a guest, in the nests of *Monomorium minutum* var. *minimum* (see p. 539).

4. The single colony of the Mexican *L. petiolatus* which I have seen was living in parabiosis with species of Cryptocerus and Cremastogaster (see p. 527).

5. *L. tuberum* var. *unifasciatus* lives with the European *Formicoxenus ravouxi*, the relations between the species being, perhaps, the same as those which obtain between *Formica rufa* and *Formicoxenus nitidulus* (see p. 538).

6. *L. muscorum*, *L. acervorum*, and *L. tuberum* live as slaves or auxiliaries with the European *Tomognathus sublævis* (see pp. 70, 71).

7. *L. curvispinosus* probably performs the same rôle in the nests of *T. americanus* (see p. 715).

8. *L. tuberum* has been found associated with *Strongylognathus testaceus*. Here, too, the Leptothorax probably acts as the slave of the dulotic species (see p. 710).

9. *L. emersoni* lives with *Myrmica brevinodis* as described in the first part of this paper. The compound nest resembles that of *L. canadensis* with Cremastogaster and of *Formicoxenus nitidulus* with *Formica rufa*, but the relations between the two species of ants are like those existing in mixed nests. In one sense *L. emersoni* is the dominant species and the huge Myrmicas are its auxiliaries, or slaves; in another sense the

[1] At Colebrook, Conn., July 22, I again encountered *L. canadensis*. In this instance the workers were running about on some stones which covered a very extensive nest of *Formica rufa* var *difficilis Em.* The Leptothorax nest, which was probably compounded with that of the *rufa*, was not found. This fact is of some interest in connection with the remarks on *L. muscorum*, recorded at p. 519.

Leptothorax is a guest or social parasite resembling Anergates, though still retaining intact its own household and its domestic instincts. *L. emersoni* may therefore be said to combine in itself the instincts of ants belonging to several categories of mixed and compound nests.

The range of habits clearly indicated in this brief survey of our very fragmentary knowledge of Leptothorax species is still further enlarged if we include the genera Tomognathus and Formicoxenus, both of which are closely allied to Leptothorax. In fact, Tomognathus is indistinguishable from Leptothorax in the male sex and larval stages (Adlerz, '96). That the three myrmicine genera under consideration must have had a common origin is evident from their morphology. Nevertheless the habits of the various species are so diverse as to represent all the forms of social symbiosis except colacobiosis of the extreme type found in Anergates. It is evident, furthermore, that the ants of these genera must have originally possessed certain traits which made it especially easy for them to enter into symbiotic relations with other species of Formicidæ. I believe that we may still recognize in many of the species of Leptothorax several of these traits, such as the following:

1. The genus has a very wide geographical distribution, a prerequisite to the establishment of such numerous and varied relations with other ants.

2. The species are all of small size. This must undoubtedly facilitate their association with other ants.

3. The colonies consist of a relatively small number of individuals. This, too, must greatly facilitate life as guests or parasites in the nests of other ants.

4. Most of the species are rather timid, or at any rate not belligerent. They are, therefore, of a more adaptable temperament than many other ants even of the same size (*e.g.*, *Tetramorium cæspitum*). Forel ('74, pp. 339, 340) has shown that *L. tubero-affinis* will rear pupæ of *L. nylanderi* and even of *Tetramorium cæspitum* and live on good terms with the imagines when they hatch.

5. There is no very sharp differentiation in habits between the queens and workers of Leptothorax. This, too, should

facilitate symbiosis. The queens, as I have shown in the case of *L. emersoni*, may retain the excavating instinct and the instincts which relate to the care of the larvæ. Wasmann ('91, p. 219, footnote) saw the queen of *L. tuberum* var. *nigriceps* removing the larvæ and pupæ when the nest was disturbed, and I have recorded above a similar observation on *L. canadensis*. More recently I have seen both the winged and deälated queens in a nest of *L. longispinosus* carrying away the larvæ quite as busily as the workers. It is as Forel says ('74, p. 339): "Les ♀ vivent presque comme les ☿; elles sont seulement moins aptes au travail."

6. The similarity in instinct between the queens and workers of Leptothorax finds its physical expression in the frequent occurrence of intermediate, or ergatogynous, forms. So-called microgynic individuals, or winged queens no larger than the workers, have been frequently observed by Forel ('74, p. 341) and Wasmann ('95, p. 618) in *L. acervorum*. Those observed by the latter author also showed color transitions between the normal queens and workers. Adlerz ('86, p. 77) found microgynic individuals in *L. acervorum*, *muscorum*, and *tuberum*. Still other ergatogynous forms, which may be called ergatoid queens, are represented by the large ocelligerous workers of *L. emersoni* described and figured in the first part of this paper (pp. 434, 436). These individuals bear a striking and suggestive resemblance to the ergatoid queens of *Tomognathus sublævis* described and figured by Alderz. Wasmann ('95, p. 619) also records the occurrence of what he calls "ergatogyne Mischformen," or individuals completely transitional between the queens and workers, in colonies of *L. acervorum* and *Formicoxenus nitidulus*.

It is possible to draw still further inferences from the heterogeneous instincts exhibited by the genus Leptothorax and its allies. Viewed as a whole, these different symbiotic relations cannot be said to bear the ear-marks of internal developmental causes operating in a perfectly determinate manner. Indeed, appearances are quite otherwise and seem rather to point to indeterminate variations which have been and are still in process of being seized on and fixed by natural selection. It

must also be admitted that the same appearance is presented by the whole complex of conditions in compound and mixed nests, but the demonstration is more cogent when it can be shown that we have relations as different as those of dominant species (*L. emersoni*) and slaves (*L. acervorum*) not only in the same genus but among closely allied forms. *This fact also suggests that the instincts of the same species may be so generalized as to enable it to function like man, either as a slave or master, according to the circumstances.*

Although these considerations may seem to lack precision they certainly show that we cannot dogmatize on the inadequacy of natural selection from a study of a few highly specialized ants like *F. sanguinea* and *Polyergus rufescens*. The complicated phylogeny of the mixed nests can only be established after a patient study of genera like Leptothorax and Tomognathus. Another even more neglected group of small ants which promises to throw some light on this subject comprises the species of Monomorium with the allied genus Xenomyrmex. At present so very little is known concerning the habits of these genera that it must suffice merely to call attention to them in this connection.

It is necessary in conclusion to consider very briefly the psychological problems suggested by the phenomena of social symbiosis, since, as above stated, the cases of dulosis have led authors to postulate unusual mental powers in ants. The accounts of sane and critical workers like Forel have been distorted by the "popularizer," till one almost believes that the *ante-bellum* Southerner might have learned many things in the management of his slaves from a conscientious observance of Proverbs vi. 6. Wasmann in his numerous writings ('91, '97, '99, '99b, etc.) has undoubtedly done much, at least in Germany, towards the exposure of this pseudo-psychology and a more rational conception of ant behavior. His long familiarity with these animals and their guests has given him a singularly lucid insight into their activities. My own more limited observations on our North American species lead me to agree with him so far as the facts are concerned and many of the inferences which he has drawn from them. I am constrained to say, however,

that I cannot adopt either his psychological definitions or his psychogenetic reservations.

Wasmann seems to me unduly to expand the conception of instinct in one direction, while circumscribing it rather too narrowly in another. It is true that he distinguishes instinct *sensu stricto*, the equivalent of the term as employed by many comparative psychologists to designate the purposeful, automatic, or stereotyped hereditary activities which are performed prior to all experience and without awareness of their object, and instinct *sensu lato*, which embraces also the activities depending on the sense-experience of the individual, and all that this implies, — adaptation and choice, associative memory, etc., — activities which have come to be very generally designated as "intelligent." But he does not appear to regard these differences as sufficiently important to merit sharp distinction. Indeed, he even attempts ('99, p. 12 *et seq.*) to show that the presence of an element of experience in the associative process of an animal is not of sufficient moment to merit distinction from purely hereditary associations. This, I believe, few psychologists will admit. The detection of such a difference, however difficult it may be in practice, is surely not beyond the possibility of carefully devised experimentation and induction. And theoretically the two kinds of activity should certainly be distinguished and separately designated. Wasmann traces the non-stereotyped activities depending on choice to a "sinnliches Erkenntniss- und Begehrungsvermögen," which he regards as being the distinguishing trait of instinct. Thus he comes to include under instinct both the instinct and intelligence of other authors. I believe with Emery ('93, '98) and Bethe ('98) that Wasmann has overshot the mark and attempted to include too much in his conception of instinct. I should continue, therefore, to emphasize the difference between activities which are compelled by inherited mechanism and those which imply choice on the part of the individual organism. For the latter the term "intelligence" has been so very generally used that it seems both hopeless and idle to try to restrict it, as Wasmann so emphatically desires, to the ratiocinative process in its clearest manifestations.

That instinct activities and activities implying choice should not be included under the same name is also evident from the difficulties which we experience when we attempt to show how the former could pass over into the latter; although the ingenious hypothesis of Spencer, James, Morgan, and Whitman ('99, p. 333 *et seq.*) may indicate where we are to seek for this transition, which these authors find in the progressive complication and mutual interference of instincts. Such conditions, it is claimed, must lead to a diminution in the automaticity of instinct and the supervention of a state of hesitancy and choice on the part of the organism.

While on the one hand, as above stated, Wasmann improperly expands the conception of instinct by including in it also the simpler manifestations of intelligence, he narrows it in another direction when he attempts to distinguish rather too sharply between reflex action and instinct. His criterion that reflex action depends essentially on the function of subordinate ganglia, whereas instinct depends on the activity of a brain, or sensorium, can only be maintained if the conception of instinct is restricted to the Metazoa and understood as including intelligence (*sensu auctorum*). But with the rejection of this definition of instinct we must also reject such a distinction between reflex action and instinct.

It may be said in this connection that the attempts of others to distinguish between instinct and reflex action are almost equally unsatisfactory. This is true, *e.g.*, of the distinction emphasized by Romanes, when he says ('95, p. 12): "I endeavor to draw as sharply as possible the line which *in theory* should be taken to separate instinctive from reflex action; and this line, as I have already said, is constituted by the boundary of non-mental or unconscious adjustment, with adjustment in which there is concerned consciousness or mind." It is well that Romanes has stamped his distinction as a theoretical one, for its application in comparative psychology is obviously impracticable, since it must fluctuate with our opinions concerning the presence or absence of consciousness in different animals. It is not at all certain that consciousness is present in the cases of pure instinct; or, if present, it may exist as a

mere epiphenomenon as Morgan explains ('00, p. 208): "An organism — if such exists — in which all the activities are throughout life purely automatic and purely instinctive, might indeed be conscious, but its consciousness would be of no practical value; for all the activities being, *ex hypothesi*, automatic, there would be no conscious guidance or control. Consciousness might be present as a spectator of the activities, but it would be a mere spectator without power of guidance, since, in so far as guided by intelligence, activities cease to be instinctive. It should be clearly grasped that, in so far as an activity is guided by individual control towards more complete accuracy, just so far does it cease to be instinctive, as the word is here used, and become intelligent. And when an instinct is, as so often is the case, modified and adapted to meet new circumstances, the modification and adaptation is no part of the instinct as such, but is due to intelligent control.

"I repeat, then, that in instinct as such consciousness is an epiphenomenon or adjunct. But this does not, of course, imply that it is absent. Only in so far as consciousness accompanies the performance of instinctive activities can intelligence get a hold on them for the purpose of control and guidance. The performance of automatic activities affords to consciousness data, which form a foundation upon which the psychical structure reared by intelligence is based."

Finally, the distinction noted by Spencer, Morgan, and others that reflex action is "localized response involving a particular organ or a definite group of muscles initiated by a more or less specialized external stimulus," whereas "instinctive activity is a response of the organism as a whole, involving the coöperation of several organs and many groups of muscles," implies only a difference in degree as Marshall ('98, p. 100 *et seq.*) and Loeb ('00, p. 77) have pointed out; for when we extend our view to simple as well as complex organisms, and forget for the moment the staple experiments of neuro-muscular physiology, we must agree with James when he says ('90, p. 384) that the "actions we call instinctive all conform to the general reflex type," and with Marshall ('98, p. 100) when he says: "All

instincts appear as modes of that simplest of all forms of activity, the reaction of a living cell to the stimulus received from its environment." And we are led to conclude with Loeb ('00, p. 177) that "the discrimination between reflex and instinctive actions is chiefly conventional." The work of this last author, above all others, is of great value in freeing us from some of the traditional misconceptions of instinct. He has succeeded in tracing a number of instincts to simple tropisms (or taxes) and has shown good reasons for maintaining that many of the more complicated instincts are only catenary reflexes (Kettenreflexe). These, however, have not yet been sufficiently analyzed.

Loeb's conception is also fruitful in another direction, for as Mach says ('00, p. 64) it throws light on the relations between development and instinct. Numerous "instincts" of ants, such as their reactions to moisture, heat, light, and contact, are evidently simple reflexes and may, I believe, be treated as cases of hygro-, thermo-, helio-, and thigmotaxis. The first and last of these reactions are especially striking. Similarly the olfactory reactions, which are such an extremely important factor in the lives of ants, are probably not essentially different from the chemotactic reactions of simpler organisms. The consociation of ants in mixed and compound nests is undoubtedly dependent to a very considerable extent on olfactory reactions. The young auxiliaries that hatch from stolen pupæ are at once adopted in the mixed nests because they have acquired the nest odor of the dominant species. Moreover, the fact that these intimate relations are established only between ants of rather close taxonomic affinities is probably due to their having very similar odoriferous secretions to begin with. The animosity of ants seems to be at once excited by species which emit peculiar or unfamiliar odors. While I accept Loeb's conception as simplifying to a considerable extent the problem of instinct, I cannot suppose with Bethe ('98, '00) that the behavior of ants and bees is entirely of a reflex nature; I can only indorse Wasmann's ('99b) and Forel's ('00–'01) comments on this author's extreme views.

Wasmann ('91, p. 179 *et seq.*) has shown in detail why it is

quite unnecessary to assume the existence of anything beyond instinct and simple intelligence in the ants which form compound and mixed nests. I should even be inclined to place a more moderate estimate than Wasmann on the psychical endowments of these animals. The manifestations of intelligence are very feeble, as any observer who tries to free himself from anthropomorphism will surely find. There are distinct traces of associations with indications of some permanence of these associations, or what might be called animal memory as restricted in its meaning by Thorndike ('98, pp. 98, 99). Imitation is clearly manifested, but in a form which does not necessarily imply the existence of consciousness. There is a certain ability to profit by experience, and considerable power of adaptation to new circumstances, both remarkably developed as compared with these powers in other insects. There is evidence of choice and of that which it necessarily presupposes, *viz.*, will, but there are no evidences of anything resembling abstract thought, cognition, or ratiocination as manifested in man. Nor are there the slightest grounds for postulating the existence of these powers, which would be a hindrance rather than a help in the activities of ants under existing conditions.

Having arrived at the same conclusion as Wasmann that there are no evidences of ratiocination in ants we have reached the limits of our brief inquiry. This conclusion, however, even if it be extended so as to exclude all animals except man from a participation in this faculty, does not imply the admission of a qualitative difference between the human and animal psyche, as understood by Wasmann. Surely the sciences of comparative physiology, anatomy, and embryology, not to mention paleontology, distribution, and taxonomy, must have been cultivated to little purpose during the nineteenth century if we are to rest satisfied with the scholastic definition of ratiocination as an adequate and final verity. And surely no one who is conversant with modern biological science will accept the views that the power of abstract, ratiocinative thought, which is absent in infants and young children, scarcely developed in savages and highly developed and generally manifested only in the minority of civilized men, has miraculously sprung into

existence in full panoply like the daughter of Jove. Such conceptions recall the rhetorical figure which Houzeau ('72, Vol. II, p. 264) aptly uses in his discussion of this same matter: "The spark," he says, "which we draw from a rod of wax is analogous to that from the Leyden jar and we attribute it to electricity. Had we from the very first sought to liken it to the thunderbolt, the difference of proportions, the quantitative inequality might have been such that we should have been scandalized by the comparison." But we need not dwell on mere opinions respecting the status of ratiocination in the animal kingdom. That the task of tracing reason to more generalized and primitive psychic processes is not impossible is shown by Binet's recent investigations ('01), the gist of which is included in the following quotation (p. 159): "There is no decided difference between perception and logical reasoning; the two operations are both reasonings, transitions from the known to the unknown. The analogy is so close that we were able to compare perception with formal reasoning, and to show that perception contains all the essential elements of a peripatetic syllogism (see p. 88). In short, perception and logical reasoning are only the two extremes of a long series of phenomena, and when we place ourselves in the middle of the series we find inferences which belong to both at the same time (see p. 70). Further, we have shown that a kind of filial relationship exists between perception and the reasonings of conscious logic. Thus when we make systematized anæsthesia, which has been developed in a patient relatively to a certain person, gradually disappear, the thing which appears first of all is the perception of the person as species; and it is only afterwards, by a kind of ascending evolution, that the recognition of the person as individual takes place; now, we know that recognition is a complex operation which touches closely upon reasoning properly so called. All these reasons lead to the belief that perceptive reasoning and logical reasoning imply the same mechanism (see p. 77)." A somewhat similar conclusion respecting the derivation of ratiocination is reached by Wundt ('01, pp. 342, 395).

However doubtful we may be of the complete success of attempts like that of Binet, we may be confident, nevertheless,

that they contain the germs of promise, for the consensus of the biological sciences leads us at last to one point of view: "We are prepared," as Mach says ('98, p. 235), "to regard ourselves and every one of our ideas as a product and a subject of universal evolution; and in this way we shall advance sturdily and unimpeded along the paths which the future will throw open to us."

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NOTES ON LIVING NAUTILUS.

BASHFORD DEAN.

DURING a recent visit to Manila I learned from Commissioner Dean C. Worcester that Nautilus, a form which one usually associates with remote and cannibal islands, could be collected quite readily in the straits between the islands of Negros and Cebu. This locality was accessible, and a short stay there, I was told, would afford one an opportunity of examining this, the only living picture of orthoceratids and ammonites, to say nothing of its greater interest as the probable key to the puzzles of cephalopod descent. So it came about that I visited Negros. Thanks to the kindness of Professor Worcester, the trip itself proved a zoölogical excursion *de luxe*, for he secured for me the services of his long-time guide, the taxidermist Mateo Francisco, and he put us in charge of his good friend, Don José Bocanegra, whose large sugar estate is near the town of Bais. It was during this trip that the following notes and sketches were outlined. They have been allowed to stand practically as jotted down, as first impressions, — with my apologies, and a dedication, if they are worth one, to the student of Nautilus, my good friend and former colleague in Columbia University, Dr. Arthur Willey.

Nautilus is doubtless common throughout the waters of the southern Philippines.[1] It can, however, be obtained so readily in the region of southern Negros that such a station, if only because of its accessibility, deserves to become a classic one. The reason why a deep-water form like Nautilus can be secured with little difficulty here is a simple one: it is a regular by-product, so to speak, of the traps of the fishermen. Its

[1] I note, in passing, that dead shells of Nautilus occur not uncommonly as far north as Japan. One was picked up a few years ago near the laboratory at Misaki (lat. 35° 10′ N.).

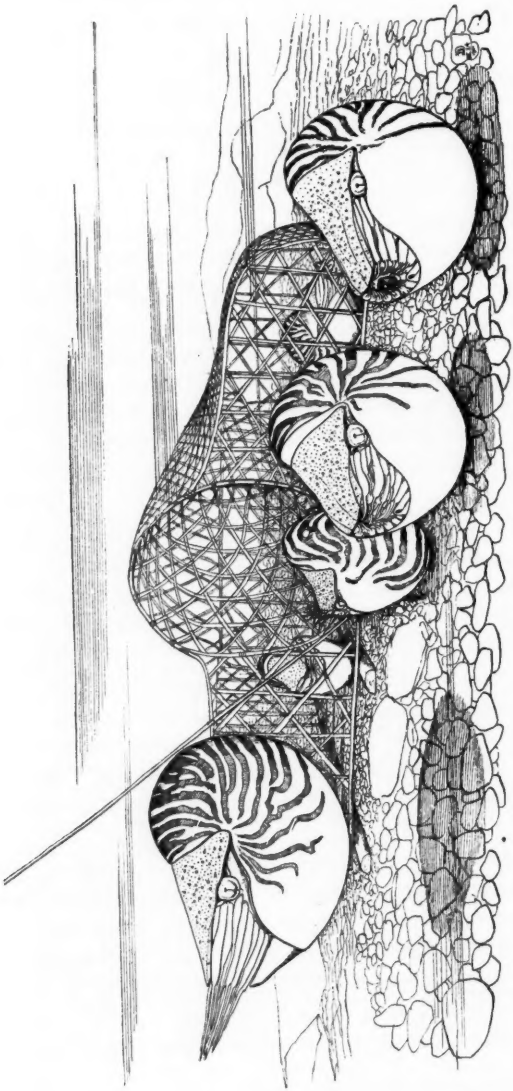


FIG. 1. — Sketch illustrating living Nautilus. In the background the Philippine fish-trap in which they are taken.

flesh is, indeed, eaten, but it is little esteemed. The shells have hitherto found little market; locally they are used as drinking cups, or flower vases, and are sometimes cut into roughly shaped spoons. But they are now bought extensively by Chinese shopkeepers, and at such good prices that there will probably be a goodly Nautilus fishery before long. The Chinese, I was told, export the shells to China, where they are used as material for button-making.

The best catches of Nautilus are made in deep water, that is, according to the estimates of the fishermen, at a depth of from 250 to 400 braças (= about 450–700 meters). I notice, however, that in the Spanish charts the maximum depth recorded is 200 meters, and it is probable accordingly that either the fishermen make extraordinary allowance for the sag of their line, or that the measurement given on the chart represents the maximum sounding limit of the Coast Survey steamer. As the water shallows, Nautilus becomes less abundant : in water of 100 meters it is still taken, although in small numbers. A single specimen, as Señor Romero informed me while at Bais, was collected in that bay by the native divers while gathering pearl oysters. There can, I believe, be little doubt of this instance, for my informant is well acquainted with Nautilus, and states that the animal was brought to him living. It was obtained in water less than 4 m. in depth.

The most favorable station thus far discovered is, as Professor Worcester stated, near the small fishing village Manjuyod, about six miles distant from Bais. This and other localities where I learned that Nautilus is taken are indicated in the accompanying outline map (Fig. 2). That it is taken off the villages of Cebu, Malaboyoc, Ginatilan, and Sambuan, I know only from hearsay. I was told that a Chinese shopkeeper made a tour of these places and collected 3000 shells; and he reported that the fishing was done not far from shore. In the locality indicated, near the pueblo of San Juan on the southern coast of Siquijor, Nautilus is taken in relatively shallow water, 100 m. and less; here, however, its abundance is inconstant. In this neighborhood I picked up a shell 3 cm. in diameter.

Season of the year has an important bearing upon collecting. Spring and early summer are the most favorable, probably because the water is quiet in the absence of the northern monsoon. June is generally stated to be the month in which

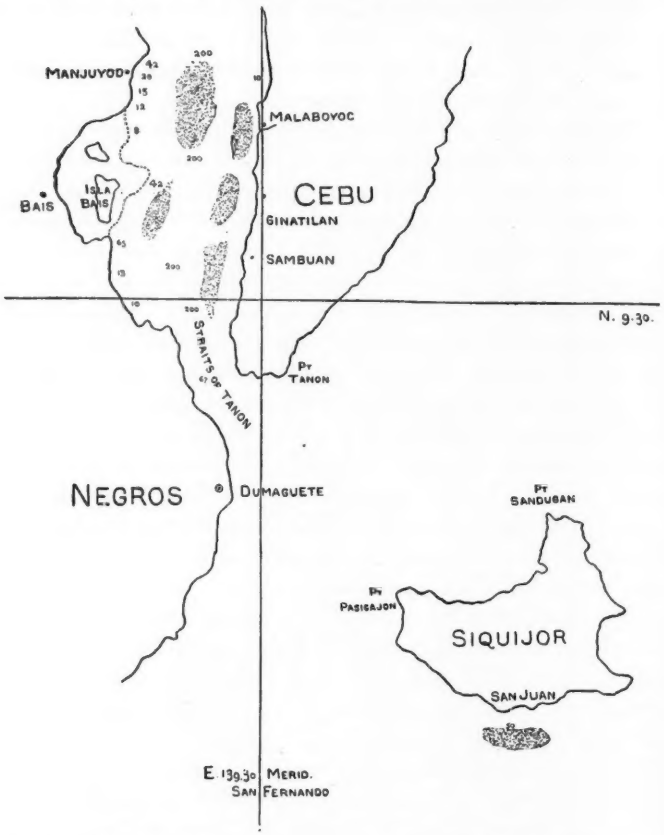


FIG. 2. — Outline map of the neighborhood of Bais, southern Negros, showing in stippled areas some of the fishing grounds for Nautilus. (After Spanish coast chart.)

the largest catches are made, as many as twenty shells having been taken in a single basket. The least favorable season is winter, for the wind sweeps down the straits between Negros and Cebu, raising a sea which soon swamps the small prahus of

the fishermen. It happened, therefore, that at the time of my visit, in January and February, I was able to get but few specimens.

The fishing, as has already been stated, is carried on by means of a fish-trap, a sketch of which is given in Fig. 3. It is a cage-like, flattened affair, about six feet in length, constructed of strips of bamboo woven in coarse mesh. At one end there is a funnel-shaped insinking through which the animals gain entrance. The funnel operates somewhat on the plan of the mouth of a rat live-trap, the animal pushing through it, not indeed at the pointed end, but on one side (the median ventral line, to borrow a phrase). Intercrossing strips then

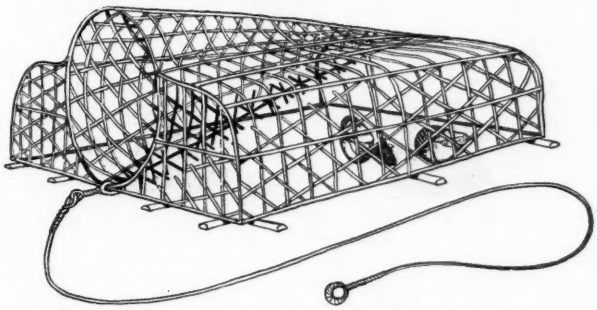


FIG. 3.—The Negros fish-cage, or "bo-bo," in which Nautilus is taken.

close elastically and prevent egress. The cage stands upon stout strips of bamboo, to which weights are usually attached. Bait is hung from the roof supports at various points. This consists of whole fowls, viscera or joints of kid, or the remains of dogs or cats when the latter attractions can be found. Freshness of bait is not regarded as a *sine qua non*. The cage "bo-bo," as it is called in Viscaya, is attached, in the way shown in the figure, by a strip of rattan, to the main line, also of rattan, which connects it with the float. The latter consists usually of several joints of bamboo. As to the method of fishing, it appears that the bo-bo is put down at a favorable opportunity, and is allowed to remain for several days, often for a week or longer. At San Juan I found that it was customary to examine the cages on stated days, weekly or

bi-weekly; at Manjuyod, on the other hand, visits to the cages are made at irregular intervals. The bait is then renewed, the captured fish removed, — this through a trap-door in the bottom, — and the apparatus sunk again to the bottom.

In the accompanying cut (Fig. 1) I have attempted to represent a bo-bo in position, with Nautilus in and around it. This, I believe, gives a moderately accurate idea of some of the positions assumed by the animals. Several are represented, partly

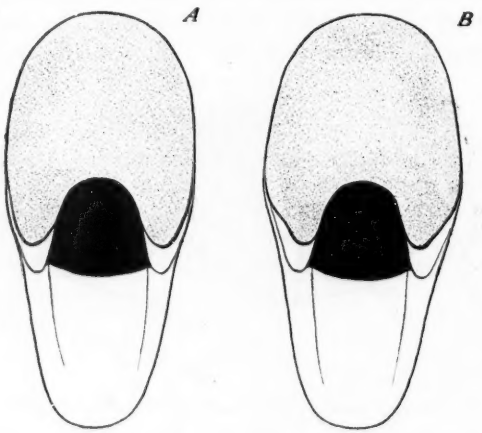


FIG. 4, *A* and *B*. — Diagrams of shells showing in stippled areas the oral apertures of male (*A*) and female (*B*) Nautilus.

retracted, in a position of rest, and one of them, attracted by the bait, is shown swimming rapidly near the bottom somewhat in the direction of the cage. The degree of shadow is, of course, hypothetical, and it is doubtful whether the bottom is as free from other living forms as I have indicated.

The Shell. — In examining fresh specimens I noticed that there appeared to be sexual differences in the shells. That of the female was wider at the sides of the oval aperture and here showed a somewhat angular contour. This peculiarity I have expressed in the accompanying Fig. 4, *B*, and it may be contrasted with the condition of the shell of a male shown in the

same figure, *A*. This difference was clear in the case of the six specimens I examined; but I later found that there must be considerable variation, for in looking over the shells obtained at Manjuyod I was unable to distinguish those of the females from those of the males in as many instances as three out of ten.[1]

Another character of the shell is the great number of irregular growth lines which they often present (Fig. 5). These lines show frequently, as in the figure at *, an undulation of a somewhat regular pattern, reminding one of the markings

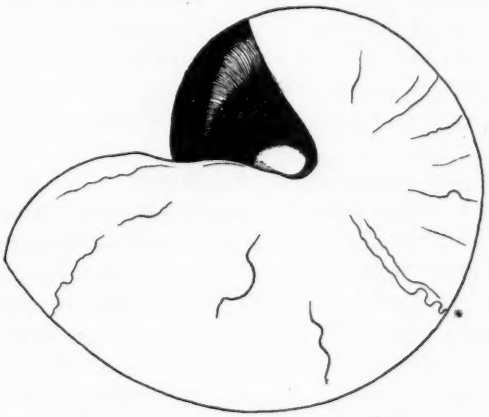


FIG. 5. — Shell of Nautilus. Outline sketch showing undulatory lines of growth.

formed at the septal rims in Ceratite or Goniatite. Doubtless this peculiarity in recent shells has already been commented upon, and the point made that the irregular growth lines cannot be directly related to those of the fossil forms, since they are not connected with the formation of septa, several being sometimes found on the wall of a single chamber. They are interesting, however, I suggest, as representing a tendency during special periods of shell-forming activity, perhaps after

[1] I find that Willey gives (1895, *Nat. Sci.*, p. 412) a similar note, stating that "in most adults the shell can readily be identified as belonging to male or female, but often this identification is very difficult, and in young shells impossible." He figures extremes of difference.

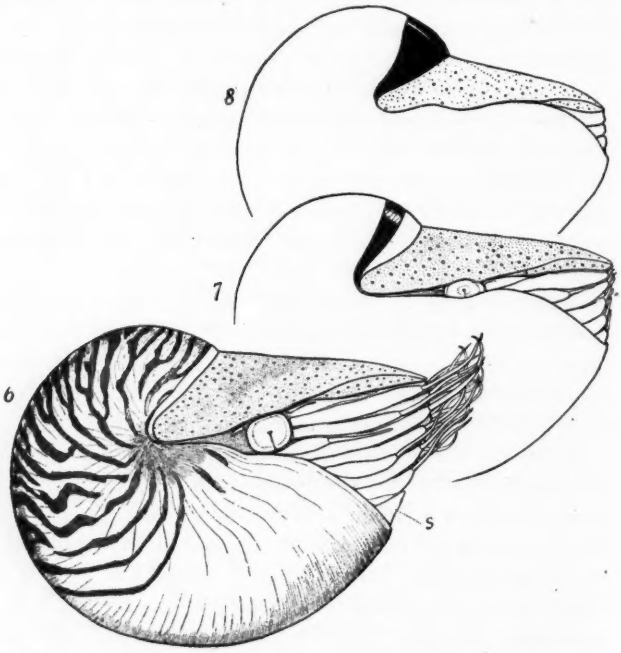
some unusual shock has been given to the free rim of the shell, for the mantle to contract in crenulate lines, a tendency which during the descent of the tetrabranchs may well have been seized upon by selection and made of use in the formation of the specialized margins of the septa. And from this standpoint the recent markings may be regarded as related to the curiously expressed lines on the ancient shells. They seem of entirely too regular a character, as close examination shows, to be interpreted merely as lines marking the repair of the free lip of the shell. Such lines of repair do, indeed, occur and are common, for the shell-lip is delicate and often apt to be injured in an animal with the evident habits of nautilus. I have in mind one instance when the shell had been repaired, after the lip had been crushed badly; in this case the main fracture passed backward from the rim of the lip to a distance of 4.5 cm.

General Appearance of the Living Animal. — The first glimpse at a living specimen showed it at the bottom of the vessel in which it had been brought, its position upright somewhat as shown in the present Fig. 6. If the vessel is sufficiently large the animal is usually found with its back (*i.e.*, its aboral pole, so to speak) pushed as far as possible into a corner. And if several specimens are thus confined they will often be found to have backed away from one another as far as possible. They remained thus almost motionless sometimes for hours; at other times they will exhibit active movements and subside quite suddenly. The color of the animal one gets little idea of from preserved specimens. The general color of the exposed parts, hood excepted, is white, clean, opaque, almost the same tint, in fact, as the body of the shell. The latter was in every case I observed brilliantly clean. The face of Nautilus, then, is white, as is also the dorsal fold of the mantle, which rises into sight within the concave hinder rim of the hood and covers when extended the jet-black portion of the coil of the shell. The dorsal surface of the hood, as one would infer from preserved specimens, is of a brownish color. Looking at this surface for the first time in a living specimen, one is given the impression that it has been spattered with burnt umber, raw

sienna, and yellow ochre, and that the pigment has then been carelessly wiped away, leaving the warty prominences in white relief. Nowhere are observed the characteristic chromatophores of the dibranch. The only pigmentation which may further be noted is on and around the eye, and in the creases between the thickened proximal portions of the oral tentacles, also on the edge of the mantle, margining the oral aperture; from this the shell itself receives an inky border, which is continued from the blackened portion of the coil around the entire aperture. This line measures about 2 mm. in width and can be detected from the outer surface of the shell; it does not appear, however, in shells which have been carelessly cleaned.

Nautilus, as far as my experience goes, lives but a short time in confinement, twenty hours being the longest time I was able to keep one alive. And even from the beginning the animal was evidently not at its best. It is true that the conditions under which I observed them were unfavorable, for in no case were specimens brought me which had been confined less than three hours in a water bucket. And the best aquarium I could improvise was a tank which held only about two hundred liters of water. Changing the water at frequent intervals had little effect in keeping the animals alive. I soon found that it was difficult to know just when the animal did die, for it retains a very lifelike position. This I discovered as follows: with a view of finding whether at any time movements became more active I caused my helper to watch a specimen through out the night, the man to call me in case it showed change of position or any peculiar behavior. In the early morning I was told that the animal had shown no movements, a statement I soon had little reason to doubt, for I found my Nautilus dead. On returning it to the water I observed that it floated, retaining, however, its customary position; and so it stayed in spite of my efforts to cause it to sink. This is the only case in which I observed a specimen remain at the surface; and the thought suggested itself that it was from such floating specimens that the idea originated that Nautilus was more or less a surface form.

The usual position and appearance of nautilus in confinement are sketched in Fig. 6.[1] It will be noted that the lower rim of the hood lies in approximately a horizontal position, the dorsal



FIGS. 6, 7, 8. — Diagrams showing the position of Nautilus at different stages of closing.

fold of the mantle protruding slightly above the concave hinder rim, entirely concealing the black portion of the shell. The

[1] In the photograph which Willey has given (*Q. J. M. S.*, 1896, p. 179), the living specimen appears to be somewhat droopy, or perhaps it is just recovering from its alarm at being confined in so small a vessel, evidently a museum jar. Thus the base of the hood has shrunken away from the shell, especially at the sides (but then this is not unusual), and the dorsal fold of the mantle which normally rises to the upper margin of the black area of the shell can hardly be seen; the tentacles, too, sag down in a way which I think rather uncustomary, cramped, perhaps, by the smallness of the glass vessel. My impression, too, is that they have drawn down the ventral lip of the shell somewhat lower than is usual. How easy it is, though, to criticise a zoölogical result, even in the form of an admirable photograph, and to forget that it may have been my own specimens that were sickly!

eye protrudes conspicuously, but not, however, beyond the plane of the surface of the shell; its lower rim lies more or less within the lateral indentation of the shell's aperture. This indentation, as will later be seen, is apparently a provision to enable the animal to see, even when largely retracted. The tip of the siphon is also seen to protrude (*S*), and between it and the hood lie the closely arranged oral tentacles. When the animal is in this position the proximal ends of the tentacles appear long, narrow, and moderately well rounded; the distal portions taper out delicately, the tips extended and weaving together, somewhat as shown in the figure. Examining a similar specimen from in front (Fig. 10), we observe a more or less regular arrangement of these tips of the tentacles; they cluster together, lying in the main in front of and almost concealing the opening of the mouth. In this position the shape of the dorsal surface of the hood is well shown, there is a median flattened tract from which the sides shelve away, moulding the hood to the whorl of the shell and to the region of the eyes and tentacles. Looking at a specimen from above, we obtain the appearance shown in Fig. 12. This, however, pictures a more retracted stage. It will be seen that the hood is neatly adjusted to the outline of the aperture of the shell. Anteriorly the distal ends of the tentacles protrude in a tuft and are arranged quite symmetrically.[1]

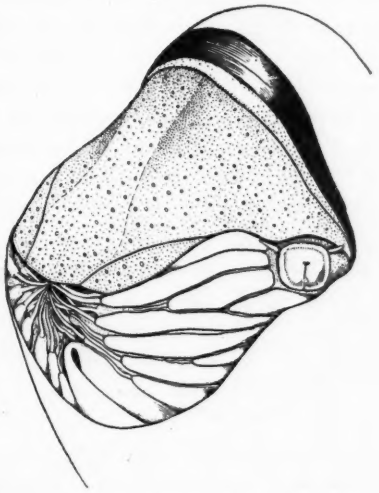
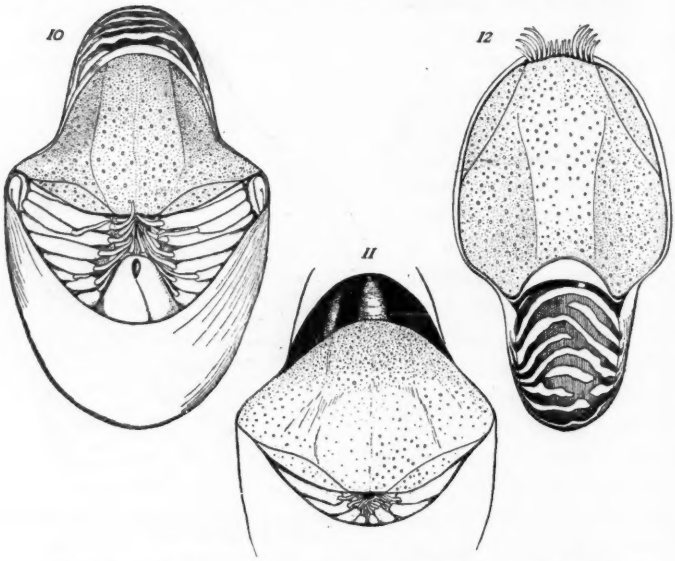


FIG. 9. — Nautilus somewhat retracted, in resting position. Oral region, shown in three-quarter view.

[1] Willey (*Q. J. M. S.*, 1898, Pl. X) figures them in complete extension, diverging widely at the sides of the head, and still symmetrical in position.

Appearance when retracting. — When a specimen is picked out of water (and it bears handling with considerable composure) it slowly retracts, presenting the appearance shown in side view in Figs. 7 and 8, in three-quarter view in Fig. 9, in dorsal view Fig. 12, and in oral view Fig. 11. The first symptoms of retraction are seen in the shortening of the tips of the tentacles and in the drooping of the hood. These processes



FIGS. 10, 11, 12. — 10, Nautilus, front view, showing oral region, at an early stage of retraction. 11, Oral region at a late stage of retraction. 12, Dorsal aspect of Nautilus resting and partly retracted, showing tentacles.

are more or less rapid, depending upon the condition of the animal; and I am led to believe that under normal circumstances retraction can take place very quickly. In the shortening of the tips of the tentacles the fleshy proximal portions become notably stouter, at the same time more angular in section, dovetailing one beside the other so tightly that their common outer surface is flattened where it is later to be opposed to the inside of the shell. Observe also that the hood becomes notably depressed, at the same time exposing the dorsal rim of

the mantle; this then recedes, exposing the blackened portion of the shell. The eye is still seen peering over the notch at the lip of the shell. In this stage of retraction the animal will often remain for many minutes; if handled somewhat roughly it will retract still further, presenting a stage of closure shown in Fig. 8. Here the hood has closed over the aperture of the shell like a lid, or rather like an operculum; it has shrunken and flattened somewhat at the same time; behind it the mantle has receded, leaving the blackened area of the shell much in evidence. At the anterior end (Fig. 11) can still be seen the turgid ends of some of the tentacles. The final closure of the aperture of the shell appears to take place by the bending down of the anterior flap-like portion of the hood, a process, however, which does not readily occur. Only by sharply stimulating this region have I seen total closure, and even then but for a few moments. If undisturbed, even if kept out of water, the animal slowly reopens. The hood first rises sluggishly, often separating itself behind from the shell, and the tentacles begin to protrude. More slowly does the mantle expand again over the blackened area. If still kept out of water the tentacles protrude and hang down over the edge of the shell in a flaccid mass.

If an animal be taken out of water and held in an inverted position it will at first close just as has been described. After some time, hours (Professor Worcester tells me) in the case of specimens just out of water, minutes in the case of those which have been kept in aquarium, the heavy fleshy "head" of the animal will begin to droop downward, and will finally appear as shown in side view in Fig. 14 and in ventral view in Fig. 15. Whether under such circumstances the animal will ultimately fall out of its shell, I neglected to observe. I found, however, that by this means one can readily remove the animal from the shell and without breaking the delicate siphuncular tube. The muscles attaching the body to the shell occupy very definite areas within the latero-dorsal region of the aperture and at the sides of the coil of the shell; and these areas are landmarked quite clearly in the inverted animal. The muscles may accordingly be separated from the shell, one's

finger-tips answering adequately for the purpose. Before the operation is completed, however, the siphuncular tube should be loosened; this is effected by thrusting the hand within the shell below the siphon. At such treatment the animal retracts, but after several attempts the finger-tips find their way to the base of the siphuncle. In the inverted position the animal presents several peculiar features. The hood is bent and misshapen, and evidently possesses considerable plasticity. Adjacent to it the black portion of the shell is largely exposed, owing to the retraction of the mantle. The tentacles are tumid, retracted, and are closely pressed together. The eyes stand out prominently, like mushrooms. The siphon shows its broad separation from the tentacular portion of the head, and hangs downward as a stout, muscular mass; at its base on either side droops a thin fold of the mantle (*M A*). Of this the inky black edge corresponds to the black line already referred to as lying within the aperture of the shell. The entire mantle is now greatly retracted: its posterior ventral rim (*M P*) exposes a portion of the extensive mantle cavity, in which, although not shown in the figure, can be seen the gills. The animal while in this position is observed to give an occasional gasping movement which involves the adjacent mantle rim as well as the siphon folds; these are expanded and contracted quickly.

The Tentacles and their Movements, Extension and Retraction. — Extension and retraction play an important and constant part in the economy of these organs. The maximum extension observed is about as shown in Fig. 8; shortly after death it is possible, however, to draw out the tentacles considerably further, to the degree which I have indicated in the swimming Nautilus of Fig. 1, or in the diagram, Fig. 13, *C*. Indeed, I suspect that the tentacles can be everted even further than this, should the animal be so disposed. It is of course doubtful if this protraction can be carried to the degree which would cause the obliteration of the distinct boundary line which separates the distal from the proximal portion of the tentacles. Judged from their varied movements, these organs have come to be highly specialized in the matter of

lengthening and shortening, if for no other purpose than that of their protection within the shell. How these processes came to be evolved there is no good evidence for determining; an attractive hypothesis would connect them with the transverse foldings which permit the tentacles to serve as adhesive organs. And one can readily conceive that a process of shortening based upon the principle of transverse folds would result in the production of one highly effective fold, to the loss or detriment of the rest. This highly serviceable fold would of course be represented in the present boundary between the proximal and distal portions of the tentacle. According to such a view the proximal portion has now lost all traces of transverse segmentation, becoming, in fact, but a kind of sheath for the rest. The distal portion, on the other hand, on account of its varied movements, has still retained them. It will accordingly be seen that the slender distal portion of the tentacle is not to be regarded as the homologue of a single highly specialized sucking organ, as some writers have maintained. There is, I believe, no evidence that such sucking organs are to be looked for in the tetrabranch division of the cephalopods. On the contrary, this division, possessing tentacles many in number and adhesive at a multitude of points, could scarcely have required the specialized sucking rings which characterize the few-armed cephalopods.

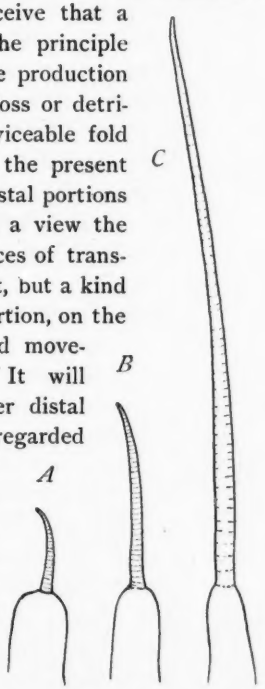


FIG. 13. — Diagram of tentacle at different stages (*A*, *B*, *C*) of protrusion.

The Movements of Nautilus. — In captivity Nautilus remains in one position, sometimes for hours. This sluggishness, however, may reasonably be due to the shock it suffers in being suddenly brought to live in surface water. Slight movements of the ends of the tentacles are sometimes the only signs that the animal is still alive. Spasmodically, however, a strength

of movement is exhibited which convinces the observer that under natural conditions Nautilus must be looked upon as an active form.[1] It will suddenly sweep from the bottom and bump its shell into a side or corner of the aquarium, — this, too, with considerable energy. At the shock the animal sinks to the bottom, partially retracts, and again remains almost motionless. In these spasmodic movements the animal rarely rises more than three or four inches from the bottom, although on one occasion I observed it swim nearly to the surface, a vertical distance, however, of not more than forty-five cm. During the night-time the movements appear to be if anything less active than during the day. A curious rocking movement is often observed, the animal swinging forward or backward, or from side to side, but never to a degree suggesting a change in the resting position. Rotation — that is, turning the shell to right or left — is very readily accomplished, and without, as far as I could see, any special movement on the part of the siphon. A very slight change in the direction of the water current exhaled from the siphon, aided by a slight leaning of the shell, is evidently enough to account for this ready movement, for the animal is delicately poised and little mechanical impulse is needed. Occasional distinct movements of the hood begin, and tentacles are observed. The hood will partly close, then open again, somewhat abruptly. The tentacles show a peculiar weaving movement; sometimes they separate quite widely from one another, and then converge. In their function as adhesive organs they will attach themselves somewhat delicately to objects presented to

[1] Professor Moseley, in his interesting *Notes of a Naturalist on H. M. S. Challenger*, observed that Nautilus swims with its tentacles "extended radially from the head, somewhat like the tentacles in a sea-anemone; but each pair has its definite and different direction, which is constantly maintained." As far as my own observations go, the exact position of the tentacles during swimming is not easy to determine, for the specimens were closely confined and their movements sudden and short; I am led to conclude, however, that the tentacles are drawn together during sustained movement, like those of other cephalopods. And in this position I represented them, somewhat stiffly, perhaps, in Fig. 1. For the above volume, together with other references to Nautilus, I am indebted to Professor Ijima, who kindly secured them for me in the University of Tokyo and forwarded them to Misaki.

them.[1] When roughly touched, however, they will merely retract. On one occasion only did I observe the separation of the tentacles around the mouth and the protrusion of the jaws, to the degree shown in Fig. 1. I was not able to induce the animals to feed. I noticed, however, that in specimens which had recently died little effort is needed to draw the jaws forward to a surprising degree, so that they would stand well outside of the proximal portions of the tentacles. And I think that there is little doubt that the jaws can be used in a position which one would

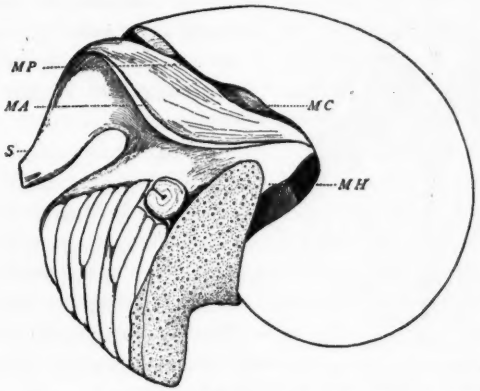


FIG. 14. — Side view of Nautilus shown hanging out of its shell. This position is assumed after the animal has been held for some time hood downward.

hardly be led to expect from an examination of preserved specimens. Accordingly I infer that the tentacles are of less importance in the mechanical operation of feeding than is popularly believed. When the jaws are drawn out, as above noted, the tongue is found to lie far forward, curving outward between the tips of the jaws. Nautilus is already well known to have a similar diet to that of other cephalopods. Its fondness for animal food in a decomposing condition I note from hearsay. I found, however, that with its strong jaws and rasping

[1] Willey in his notes convinces me that I have not examined the tentacles when at their best, for he has seen them "adhere so firmly to a foreign body as to become torn away from the animal when the foreign body is forcibly removed." (*Q. J. M. S.*, 1898, p. 207.) His figure indicates that the tentacles when firmly attached become partially retracted, at least as far as their tips are concerned.

tongue it has little difficulty in feeding upon animals which are enclosed in a stout leathery skin. Reference has already been made to the spasmodic movement of the siphonal flaps and of the ventral rim of the mantle; under normal conditions the movement of exhalation is practically confined to the siphonal flaps, the ventral rim of the mantle being closely apposed to the aperture of the shell, just as the portion of the mantle behind the hood is apposed to the black region of the coil. It is not difficult to convince one's self that under natural conditions Nautilus is well able to protrude its "head" quite widely from the aperture of the shell in a way suggested by the position of Fig. 14.

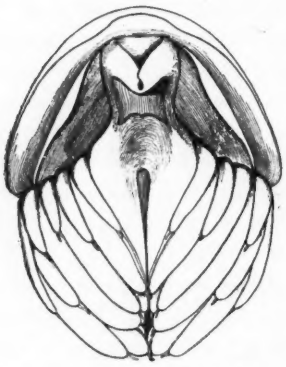


FIG. 15. — Individual of preceding figure shown in somewhat ventral aspect.

Breeding Season, and the Question whether Eggs and Young have been collected by Fishermen. — From the condition of the reproductive organs in five specimens examined I am led to infer that the breeding of Nautilus in the region of southern Negros extends over a considerable season; this evidence, however, is too meager to be of particular value.[1] There is certainly, it seems to me, a fair chance of securing the embryos of Nautilus in this region if what I learned from the fishermen is to be relied upon. One of these men in particular gave such a satisfactory account that I cannot very well doubt that he had at some time seen deposited eggs. And it may be well to state parenthetically that no data were given to the fishermen which they could make use of in their reminiscences. The fisherman in question assured me that on one occasion, as nearly as he could remember it was during the early part of the summer of

[1] I find that Willey at first believed that in the case of Nautilus in New Guinea "reproduction takes place all the year round" (*Q. J. M. S.*, 1896, p. 176). After his observations in Lifu, however, he abandoned this conclusion, stating that "it now seems probable that the breeding of Nautilus . . . is subject to a definite law of periodicity" (*Nature*, 1897, p. 403).

1900, he noticed eggs attached to a fish-cage in which Nautilus had been taken : there were several eggs, he did not remember how many, but they were attached not far from one another, and stood up on end, each as big as two joints of his finger, and with a leathery skin. Their color was white, and — this was the curious part of his story — they had brown markings across them [1] which reminded him of those on the shell of the adult animal. It was from the latter feature that he was sure the eggs belonged to Nautilus. His account seemed to me worth repeating, since it is hardly probable that he could have invented offhand so tolerably accurate a story, or that he would have been apt to describe an object which has nothing to do with eggs of Nautilus and yet resembles them so clearly. Moreover, there is a greater probability of accuracy when one considers that such a man is apt to be a skillful observer in matters relating to his work, — a thing which a stay-at-home zoölogist often finds hard to realize. I was assured on every hand that small specimens of Nautilus are relatively common during the fishing season ; that specimens whose shell is the size of a silver dollar are often thrown away, and that still smaller specimens are occasionally taken, in spite of the large mesh of the fish-cage.

[1] Could these be the rows of foldings and "fenestrations" of the outer egg capsule which Willey described (*Nature*, 1897, p. 402)?

BASELEVELING AND ITS FAUNAL SIGNIFICANCE, WITH ILLUSTRATIONS FROM SOUTHEASTERN UNITED STATES.

CHARLES C. ADAMS.

In considering some of the peculiarities of the fauna of the southeastern United States I wish to call attention to certain principles that are fundamental to an understanding of the present distribution of many forms of life, especially those of land and fresh-water. Especial stress is laid on the relation of physiographic changes, and baseleveling in particular, to faunal changes and to the differentiation of land and fresh-water animals.[1]

The necessity of correlating the results of baseleveling, and river histories in particular, with the distribution of habitats and their fauna, was very emphatically impressed upon me in connection with some studies upon the Pleuroceridæ of southeastern United States. This relation was clearly demonstrated to me when I learned that the former course of the Tennessee River from Chattanooga was direct to the Gulf *via* the Coosa-Alabama system. Having thus become fully convinced of the great biological significance of the baseleveling factors, it was of considerable interest that I later learned that Woodworth ('94) had already appreciated this significance.

In a general way, this relation of physiography to topographic and geographic distribution has been well understood, but such studies need to be carried out in more detail. The geologists, from their familiarity with the physiographic processes, seem to have had a better appreciation of this relation than zoölogists. Woodworth ('94) has fully realized in a detailed way the influence of the baseleveling processes upon faunal changes.

[1] I am under obligations to Messrs. Salisbury, Hayes, and Campbell for permission to use their figures and to Prof. E. A. Birge of the Wisconsin Geol. and Natural History Survey for the loan of the cuts for Figs. 1, 2, and 3.

His paper is of great value to the general student as well as to the special student of faunal problems.[1] I find that he had anticipated many of the conclusions to which I had come, but, on account of their imperfect recognition, they will be well worth repeating. Geologists have been primarily concerned with the submarine aspect of physiographic changes. Chamberlin has discussed this in a very suggestive paper ('98).

Since there is a very evident correlation between the erosion of the land and the deposition of the eroded material in the sea, we are able to see that under some circumstances both the submarine and subaerial factors of physiographic change must be taken into consideration in explaining habitats and the struggle for existence of aquatic organisms. This may be illustrated if we suppose that as a continental shelf becomes reduced in area the crowding of its fauna might force some of its members into fresh water, a habitat with a relatively poor fauna, which consequently is open territory.

It appears that many biologists are unfamiliar with the idea of baseleveling. For this reason the following brief account is given, showing how rivers cut down land and tend to reduce it to a baselevel. If a comparatively level country be elevated out of the sea a few hundred feet, and rains begin to work upon it, the slight inequalities of the surface will cause the waters to collect in the hollows and then run off, carrying detritus and cutting a trench or gully. This trench becomes with succeeding rains deeper, wider, and longer as it cuts its way into the uplands, and thus valleys may be formed. With this growth or increase of the area of valleys there is an increase of lowland and cliff habitat and a decrease in the upland habitat. These progressive changes are beautifully shown in Figs. 1, 2, and 3, after Salisbury. It is important to notice here the changes in the relative proportions of the habitats, with a premium placed upon those forms which can

[1] Dr. H. C. Cowler has recently applied these principles to the study of plants: The Physiographic Ecology of Chicago and Vicinity; A Study of the Origin, Development, and Classification of Plant Societies, *Bot. Gazette*, vol. xxxi (1901), pp. 73–108, 145–182.

The Plant Societies of Chicago and Vicinity, *Bull. of the Geographic Society of Chicago*, No. 2, 1901.

occupy the habitat that is increasing in area. These changes will produce tension lines between the inhabitants of the various habitats.

With regard to the river fauna, at first when the fall of the river is great, rapid-water forms have the advantage, but with the development of sluggish, meandering streams upon bottom land, quiet-water forms find a home. Since the lowland or swampy conditions begin at the mouth of the stream, quiet-

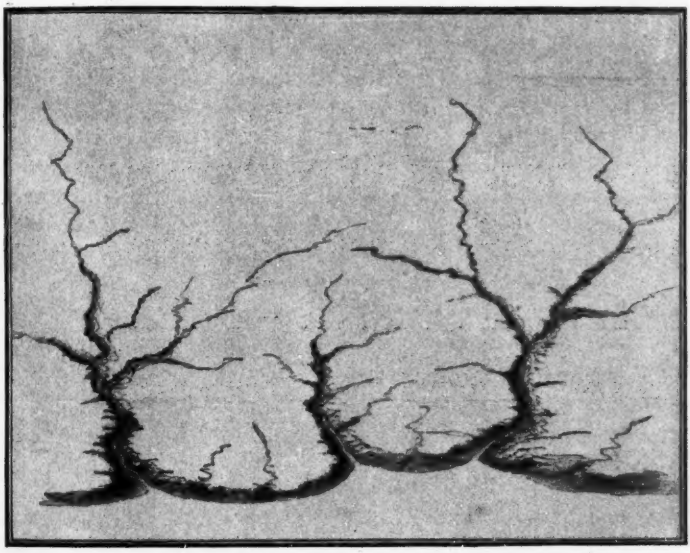


FIG. 1. — Young valleys. (After Salisbury.)

water forms will be introduced there, and with the extension up stream of lowland conditions this fauna is led in that direction; similarly, the head of the valley leads *its* rapid-water fauna in the same direction. Thus there is a definite succession of forms working up the valley, and, in a certain sense, by passing down stream one gets a recapitulation of the types of fauna which inhabit a given point of the stream during its ideal history. Dendritic tributary streams help to carry these same conditions in diverging directions, thus leading the fauna in various directions toward the divides.

Later on, with the wearing away of the inter-stream uplands, which at first were flat-topped, but which become more and more reduced by the dendritic etchings of the smaller streams, the divides gradually become more sharp, and the country

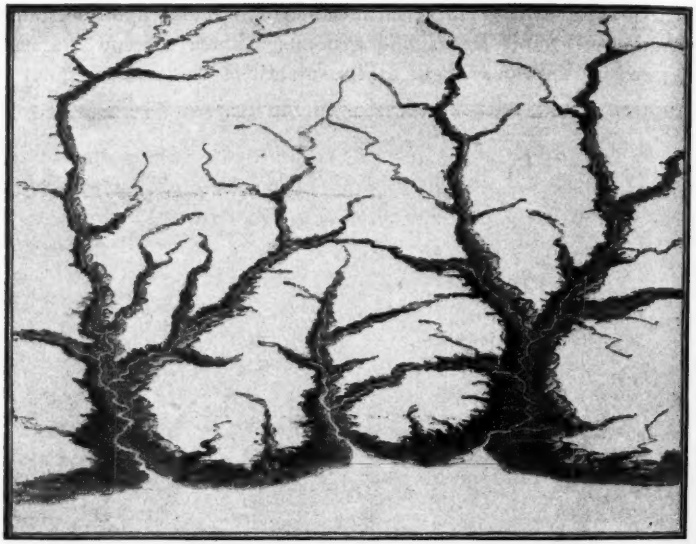


FIG. 2. — Valleys of later stage. (After Salisbury.)

reaches its maximum degree of roughness and cliff habitat, and consequently the greatest isolation of its streams and their faunas. By degrees these sharp divides are lowered, and head-water streams favorably located begin to capture fellow streams with their faunas, and thus there is a mixing of rapid-water faunas.

The time of maximum roughness in the topography coincides with that of maximum isolation, but with the fusion and concentration of drainage lines a period begins which favors wide distribution by the removal of barriers; thus in a country approaching baselevel a wide distribution of the fauna will be greatly facilitated with a premium placed upon the lowland fauna. This stage of wide distribution favors interbreeding,

and thus a period of swamping by intercrossing may be produced. The cliff fauna would have its maximum distribution and area of habitat when the country was at its maximum roughness.

It will not be necessary here to go further into the details of these processes, as the preceding remarks, with an examination of the diagrams, will suffice to show that these principles may be applied almost indefinitely, but the details vary greatly in different localities. One point perhaps needs special emphasis, and that is, we must think of these processes as *active* and taking place before our eyes.

The bearing of these factors upon the large amount of endemism seen in southeastern United States will be illustrated by a few examples. The physiographic changes of this

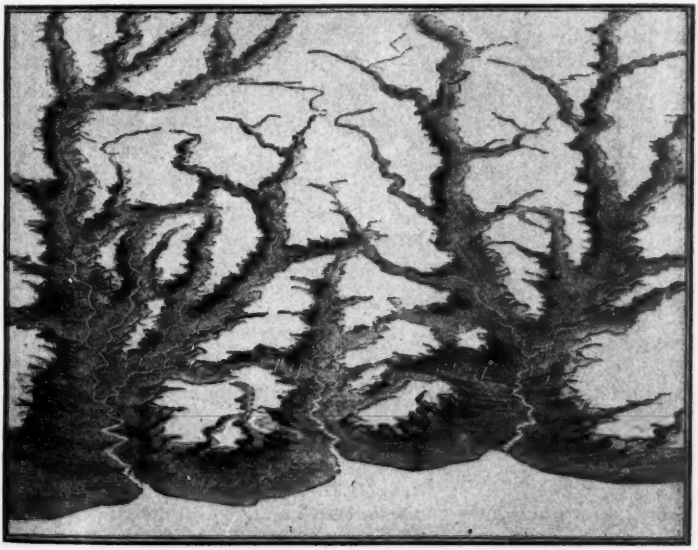


FIG. 3. — Valleys of further development than shown in Fig. 2. (After Salisbury.)

region have been very carefully studied, but comparatively little has been done to correlate these changes with the geographic distribution of the fauna of this region, and especially with its endemism. I hope, by calling attention to a few of these

correlations and to some of the principles involved, that others, more familiar with special groups of animals, may be led to make similar comparisons and correlations, and also to emphasize the fact that causal and genetic faunal studies must in the future take into account these factors, if work of permanent value is to be done. Biological surveys will, carried out in this way, become decidedly more than faunal lists, with miscellaneous biological information. In order to emphasize certain aspects of the baseleveling processes special phases will be discussed under separate heads.

Migration of Divides.

The migration of divides has long been recognized as an important factor in the mixing of river faunas, but without a distinct recognition of it as a phase of the baseleveling process. The following examples will show the influence of the migration of divides upon faunas. But first, attention will be called to some of the facts of the distribution of the family Pleuroceridæ, or Strepomatidæ of the older authors. This family is distinctly North American, living forms occurring here only. Tryon's monograph ('73) on this family, from which I take my data, recognizes four hundred and sixty-four species. Excepting those species whose exact locality is not definitely known and certain Pacific coast forms, most of these are limited to the Tennessee River and its tributaries above Florence, Alabama, and to the Coosa River. Two-thirds, or about three hundred, of these species are confined to these two river systems.

The region in which this family reaches its maximum development in both individuals and species is remarkably limited and is bounded by Tryon as follows. On the "*North*, by the Tennessee River and tributaries. The Cumberland Mountains prevent the dispersion of the species of this river to the northward until its course is directed into Alabama. Here the character of its species (which we will again allude to further on) changes, and they become gradually less numerous and of greater geographical dispersion as the river runs toward the west. *East*, the mountain range of the Blue Ridge, running

southwestwardly into the interior of northern Georgia; thence, the Chattahoochee River and tributaries, to within a hundred miles of the Gulf. *South*, the species are restrained from

FIG. 4. — Map of Appalachian valley, which is the area of maximum differentiation of the Pleurocerida. (After Hayes and Campbell.)

spreading by the influence of the Gulf of Mexico. *West*, the Alabama, Cahawba, and Black Warrior Rivers and their tributaries, those of the latter reaching almost to Florence, on the

Tennessee River, which may represent the northwestern point of our boundary.

"These limits are necessarily imperfect, but, nevertheless, include at least three-fourths of our species within an area of three hundred miles extent, either north and south or east and west." By reference to Fig. 4 (after Hayes and Campbell), one will see that these limits are practically those of the Appalachian valley, and thus enclose a natural physiographic region.

Again, Tryon remarks: "Assuming the Ohio River as a dividing line, we find that ninety-five per cent of all the species originate south of it."

So much for these general facts of the distribution of this family which are necessary for an understanding of the influence of the migration of the divides upon them.

With the above facts of the distribution of these shells fresh in mind, about two years ago I learned from Hayes's ('95) paper of the view that the upper Tennessee River formerly flowed by way of the Coosa-Alabama Rivers into the Gulf. I saw at once in this an adequate explanation for the peculiar distribution of many of these shells in these two river systems. Since the data for this paper have been collected Mr. C. T. Simpson ('00) has published similar conclusions, drawn from the Unionidæ. According to Hayes's and Campbell's ('94), and also Hayes's ('99), opinion, the Tennessee River above Chattanooga was captured by a westward-flowing stream, and thus led away from the Gulf by the Coosa-Alabama to the Gulf again by way of the Ohio and Mississippi. Thus we see that the region in which the maximum development of this family is reached was *broken into two parts* (Fig. 5). The bearing of this upon the shell fauna is very marked.

This family is divided into two sections, and as to their distribution Tryon (*loc. cit.*, p. xl) says: "While the *Trypanostomoid* forms attain their maximum development in size and number in the Tennessee River, they are to a very great extent replaced by the *Goniobasic* forms in the Coosa River, which is undoubtedly the metropolis of the latter. The most striking genus of each of these groups is absolutely confined to the respective streams in which the groups had their origin.

Thus, Io and Schizostoma are inhabitants, the first of the Tennessee and branches, the second of the Coosa, and neither of them are elsewhere found." This is an excellent illustration of how the migration of a divide may favor differentiation.

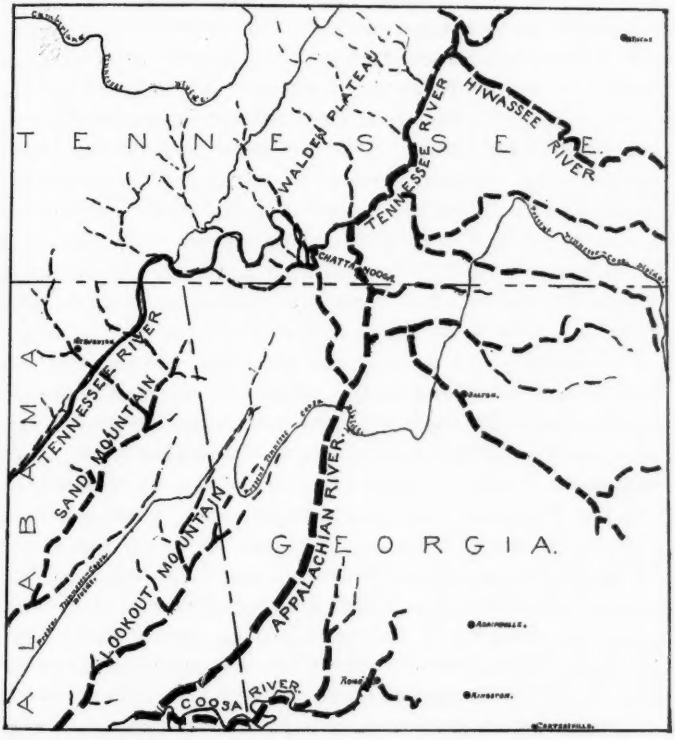


FIG. 5. — Map showing ancient (broken line) and present (continuous line) drainage in the vicinity of Chattanooga. (Adapted from Hayes, '99.)

The family has thus been broken up into two parts, both geographically and biologically.

The genus Pleurocera is credited with eighty-four species, twenty-three of which are found in Tennessee, and twenty-one are apparently peculiar to it. This same genus is represented in the Alabama system by fourteen species, and three of them are peculiar to it. "These species," says Tryon, "are generally

confined, however, to those portions of the Coosa and branches that approach to east Tennessee." Lithasia and Pleurocera are other genera divided in their distribution between the two systems. In the genus Strephobasis one species (Bitæniata) is found in the Black Warrior of Alabama drainage and in the Tennessee River at Chattanooga.

In the Goniobasic section there are two types of shells: one group is characteristic of the Tennessee and the other of the Coosa. In the genus Goniobasis, about ten of the elevated smooth species are found in the Tennessee, and about the same number in the Alabama system. Tryon (*loc. cit.*, p. xlviii) says of this genus: "There are over sixty species in the group which I have designated as 'compact and ponderous' for want of a better name. They are essentially a distinct group from the other Goniobases, and all the species except three are peculiar to the branches of the Alabama River."

Consequently, we see that this family reaches its maximum development in two distinct river systems, the upper Tennessee and the Coosa-Alabama, which formerly formed one river system (Fig. 5), and that by the diversion westward of the upper Tennessee, by the migration of its divide, this fauna has been split into two parts, each part having several genera, as well as species, in common. This process has led to an isolation of the Coosa-Alabama fauna and a mixing of the waters of the upper Tennessee with those of the Mississippi valley. In faunal studies some of the most striking peculiarities of the Tennessee River are very apt to be overlooked if we consider it a part of the Mississippi system only.

That these two systems have been separated for a considerable period of time, or that these forms have evolved very rapidly, is shown by the fact that it is the genera or groups of species which are found common to both streams rather than the individual species.

The family Pleuroceridæ is not the only group which has divided by the diversion of the waters of the Tennessee. The family Viviparidæ has a geographical distribution very similar to that of the Pleuroceridæ, its greatest variety being found in southeastern United States. The genus Tulotoma is confined

to the upper part of the Coosa River, and Lioplax has two species, one from the Coosa and the other from the Mississippi valley drainage. The species *Campeloma ponderosa* is found in both river systems.

An interesting illustration comes from crayfish. *Cambarus spinosus, extraneous,* and *erichsonianus* are found in both the Tennessee River and in the head-water parts of the Coosa-Alabama system.

A head-water fauna is very different from a lowland fauna; thus a knowledge of the habitats of animals will furnish criteria by which we are able to recognize, to some degree, the conditions under which divides have migrated. Since valleys continually lead their rapid-water fauna to the divides, these forms are the first which become transferred when they are shifted. What evidence is there to support this view? This is well shown if we consider those forms which are the *first* to surmount mountain barriers.

The Rocky Mountains have been a very formidable barrier to our fauna, there being but few passes or gaps through them. The fish fauna on each side of these mountains in the United States are remarkably distinct, but two species, according to Jordan ('96, p. 118), are found on both sides of this great barrier, and these species are very naturally rapid-water species, and not lowland forms. These two fish are the Rocky Mountain trout (*Salmo mykiss* Walb.) and the Rocky Mountain whitefish (*Coregonus williamsoni* Gir.). In the Appalachian Mountains the upland living brook trout (*Salvelinus fontinalis* Mitch.) illustrates the same point, it being found on both sides of the range.

Extension of Lowland Faunas into Uplands by Baseleveling.

River valleys are well known as highways for the dispersal of animals. These valleys, of course, are not always due to the process of baseleveling, but in the southeastern Appalachians we have good illustrations of the extension of a valley due to this process. The valley of the Tennessee River above Chattanooga is an excellent illustration of this. By means of

a narrow neck of lowland being carried back into the uplands an isolated, elongated land habitat is developed which reminds one of river isolation. Merriam's Life Zone Map ('98, plate) shows that there is an extension of the Upper Austral Zone into the Transition Zone due to the Tennessee valley. With the baseleveling process the isotherms gradually migrate up the valley and thus help to make it possible for the fauna to follow. Of course valleys, of whatever origin, may act as highways of dispersal.

It is important in this connection to recall the more or less definite succession of forms up the valley, because the problem of dispersal among some animals is largely a problem of the *continuous* distribution of the habitat.

Segregation of the Upland Faunas by Valleys.

Correlated with the extension of the valleys into uplands is the segregation of the uplands by the growth of the valley. This influence is beautifully shown by the land shell fauna on each side of the Tennessee valley. Since the Tennessee valley divides the uplands of the Cumberland plateau from those of the Smoky Mountains, I was led to anticipate that this isolation, which has existed for a considerable period of time, would show upon the land shell fauna, because these animals are very sensitive indicators of such influences. Later I was interested to learn that Pilsbry ('00) had very recently divided the "Cumberland sub-region" of Binney into an eastern and western division, corresponding to the uplands segregated by the Tennessee valley. He gives about twenty species and varieties characteristic of the Cumberland plateau, and about twenty-five to the Smoky Mountains. The French Broad River has for a long time separated the Great Smokies from Roan Mountain, and corresponding with these facts it has been found that six forms are peculiar to Roan Mountain and about a dozen to the Smoky Mountains.

Influence of Baseleveling upon Tension Lines.

The relation of the faunas of the different levels or base-levels to each other is also important from the standpoint of tension zones. On account of the large area which they may occupy at certain stages of topographic development these zones have great importance, for in them there is a large population subject to a peculiar environment and hence liable to important evolutionary changes. A stage of equilibrium is not reached here, as erosion makes this area relatively unstable and subject to continual changes. It is important to determine which is the more powerful, the physiographic or organic influences, in the production of these lines. The powerful influence of the physiographic processes in general leads one to expect that at first the physiographic factors will dominate, but later, in a fairly uniform habitat, the struggle will become more organic. In the study of dispersal and distribution of animals, it is important to see that the physical conditions lead, and that in a more or less definite succession the flora and fauna follow; thus the fauna comes to fit the habitat as a flexible material does a mold.

The time is passed when faunal lists should be the aim of faunal studies. The study must not only be comparative, but *genetic*, and much stress must be laid on the study of the habitat, not in a static, rigid sense, but as a fluctuating or periodical medium. The bearing of faunal studies upon the problem of differentiation and the origin of species, is very close, and in our search for the factors we must not lose the perspective, and overlook those factors which are fundamental and work through long periods of time.

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NOTES AND LITERATURE.

ANTHROPOLOGY.

The Seri Indians.[1] — It is especially fortunate that Seriland should have been explored by an ethnologist eminently fitted to describe the physiographic features of that little-known region. Professor McGee's memoir upon "The Seri Indians" is furnished with a new topographic map, a detailed description of the country and of its fauna and flora, as well as a succinct account of the arts and social institutions of the natives.

After presenting a comprehensive summary of Seri history, the author deals with the somatic characters of the people. The Seri differ from other Amerinds in their physiological and physical characters as strikingly as in their demotic. They are remarkable especially for their fleetness of foot to an extent that we should regard as quite incredible were it made known to us merely by travelers' tales.

The description of Seri demotic characters more nearly approaches completeness in the chapters devoted to Symbolism and Decoration, Industries and Industrial Products, and Social Organization. Personal decoration is confined almost entirely to facial painting, and that is a feminine prerogative. In discussing the "significance of decoration" Professor McGee outlines a scheme of progressive development from what he terms automacy to autonomy that is intricate but interesting. Industrial development is surprisingly low among the Seri. The "industrial use of stone is fortuitous and temporary." The author offers a new classification of primitive stone art based upon his observations among the Seri. The protolithic stage is that in which the stones are not shaped to conform to an established pattern; the technolithic, wherein the stones are fractured or otherwise shaped. Seri marriage customs are instructive to the student of social organization; they exhibit an almost unique moral test, and show a deep-seated interest in the maintenance of the tribal autonomy. The Seri are polygenous, apparently because of the reduction in the number of warriors in recent years.

[1] *Seventeenth Annual Report of the Bureau of American Ethnology*, 1895–96. Washington, D. C., 1898 (distributed 1901). 344 pp., map and plates.

In addition to the store of geographic knowledge this memoir is a positive contribution to ethnologic science of the highest value. It establishes a new linguistic stock (Serian), it affords illustrations of priscan phases of culture of extreme rarity, and the author advances many suggestions of theoretic interest.

FRANK RUSSELL.

ZOÖLOGY.

Schmeil's Zoölogy.[1] — This work was originally prepared for students in German Gymnasia and "Realschulen," and has as its primary object a disciplinary use of the facts of zoölogy. It tells certain facts, it leads the student to infer other facts and reasons by numerous and carefully worded questions. It differs from the old-time zoölogiés, which merely gave a description of selected animals, by some very important features. It takes representative forms and describes them from an œcological standpoint; showing how they are adapted to their environment, and how they are fitted to a certain kind of life. Then follow shorter descriptions of allied forms. There is a minimum of structural details throughout, but the biological side, so interesting to young students, is everywhere emphasized. We would advise that all secondary schools teaching zoölogy have a copy of this work in the reference library; its price and its unnecessarily large size forbid its use with us as a class-room text.

K.

Herrick's Home Life of Wild Birds. — The subtitle of Professor Herrick's book,[2] if it is understood that only the home life of birds is to be studied and photographed by the new method, will serve as a guide to the nature of this very valuable addition to the list of books dealing with bird life. Professor Herrick has, by the help of the strong parental instinct in birds, overcome the difficulties with which the photography of birds in the wild state has hitherto been attended. Instead of trusting to mechanical devices for arranging

[1] Schmeil, Otto. *Text-Book of Zoölogy*, treated from a biological standpoint, translated from the German by Rudolf Rosenstock, M.A., edited by J. F. Cunningham, M.A. London, Adam and Charles Black, 1901. xvi + 493 pp.

[2] Herrick, Francis Hobart. *The Home Life of Wild Birds.* A New Method of the Study and Photography of Birds. With 141 original illustrations from nature by the author. New York and London, G. P. Putnam's Sons, 1901. 148 pp.

the camera in the branches of trees, and taking a chance shot or two when the bird shyly returns to the nest, he cuts off the limb or twigs on which a nest containing young birds is fixed, and removes it to a favorable situation near by. Then, concealing himself and his camera in a small green tent within a few feet of the nest, he waits for the parents to adjust themselves to the new conditions. This they do in a surprisingly short time. In a few hours the old site is forgotten, and the birds are as firmly attached to the new one as if they had themselves chosen it. The operations of feeding, cleaning, brooding, and other incidental actions of old and young may now be observed and registered with a completeness and clearness impossible under the old method.

Professor Herrick's book contains introductory chapters dealing with the instincts and habits of birds and the psychological principles involved in the successful practice of the new method, followed by other chapters explaining and illustrating the method as employed by Professor Herrick. Seven of these are devoted to a detailed account of the home life of a number of our common birds as observed at close range. The text is supplemented by a series of photographs, which surpass for clearness and scientific value, as well as for popular interest, anything of the kind heretofore published. The birds thus treated belong for the most part to the passerine order; the night hawk and the kingfisher are the only exceptions. The very fact that in the study of such familiar birds as the robin, catbird, and cedar bird, we are offered so much interesting observation, much of it original in its accuracy of detail, gives a good idea of the value of the method, and promises rich results when it is applied to less familiar birds.

The concluding chapters of the book deal with general questions, such as the force of habit, fear, etc. There is an excellent index; the paper, typography, and the reproduction of the photographs are of the best quality. The excellence of the book in such matters is in itself evidence that it is intended not as a scientific treatise, but as a popular "bird-book," and if it is reviewed as such, it should be awarded only the highest praise. There is, however, much reference in the first and the three last chapters to the questions of instinct and habit, and they are treated in several paragraphs in a purely philosophical spirit. The author, moreover, puts in at the outset a strong protest against what he justly terms the "gross anthropomorphism" which characterizes much of what is now written on animals. A first reading of Professor Herrick's book, however, will

leave the ordinary reader with no very clear notion of the author's ideas on the subjects of instinct, habit, fear, etc. His conclusions, though in accord with such an excellent authority as Principal Morgan, are by no means clearly presented. The reader follows on one page a rather discursive account of an encounter with a black snake, and, turning the page, finds himself confronted with a difficult paragraph on the instinct of fear. The anecdotes which are to elucidate the principles laid down, or to be laid down, overweigh the reasoning, and often have no especial bearing. More systematic arrangement of ideas, an occasional paragraph recapitulating conclusions, and an introductory line here and there to show the bearing of the coming anecdote would greatly increase the value of the semi-scientific chapters. It might have been better, moreover, to avoid the dogmatic attitude of the paragraphs on the nest-building instinct on page xvi, and make reference at least to the possibility of tradition playing a part in handing on the art of nest building. The nests built by chaffinches turned loose in New Zealand (A. R. Wallace, *Darwinism*, p. 76) may be adduced as evidence that a bird like the robin perhaps does not make mud nests "as instinctively as it lays blue eggs."

Of undoubted scientific value are Professor Herrick's observations on the food offered to the young, on the way the food is carried to them, and on the sanitation of the nest. The sensitiveness of the gullet of young birds and the parent's habit of taking out food not immediately swallowed and transferring it to another gullet, throws light on what has been hitherto an obscure process. It is doubtful whether the home life of other passerine birds will vary much from the types observed by Professor Herrick, but there is, as he suggests, a great unexplored field in the orders.

It is as a popular but unusually accurate and thoughtful presentation of a subject at present much in vogue that the book has a special claim to recognition. Professor Herrick's patience, ingenuity, and quick power of observation deserve the success with which the book is sure to meet. The wealth of extraordinarily good illustrations, the intimate relations with birds into which the reader is brought, will fascinate any one who has any interest in the study of living animals. If the author's style lacks the inspiration which a more imaginative temperament might give to it, his scientific habit of mind, joined to his evident enthusiasm, make him an excellent guide and model for those who may begin observation by his method. The method is admirably suited for instruction in summer schools,

and has been already adopted in at least one outdoor school of natural history.

The objections to the method arising from the possibility of danger to the young thus removed from the site selected by the parent are carefully discussed by Professor Herrick. It is well, however, to repeat his warning against interfering lightly in the home life of wild birds. In the opinion of the present writer, none but trained naturalists should use the method, for they, if animated by Professor Herrick's genuine love for the individual bird, will be on their guard against the dangers likely to be incurred.

R. H.

The Fishes of Ohio is the title of a paper by Professor Raymond C. Osburn, published as a *Bulletin of the Ohio State University* (Ser. 5, No. 20). The paper is a descriptive faunal list of the fishes known to occur within the borders of the state. In the Introduction is given an historical sketch of ichthyological investigation of Ohio fishes.

Each species is briefly, though sufficiently, described; following each description is a list of localities in the state where the species is known to occur; there is also given a few notes concerning its habits, etc. Keys to facilitate identifications are also given. The paper is neatly and carefully gotten up and indicates very careful and thorough work.

The publication of descriptive faunal lists like the present one, by institutions which have the facilities for such work, is to be highly commended.

In a footnote *Notropis fretensis* (Cope) is regarded by Mr. Osburn as being allied to *Notropis heterodon* Cope and *Notropis cayuga* Meek, a belief shared by Drs. Jordan and Evermann. It is, however, a *Notropis rubrifrons* (Cope), differing from the typical *rubrifrons* in having but eight anal rays. In his original description Professor Cope calls attention to the fact that this species resembled *Minnilus*. *Notropis rubrifrons* usually has ten anal rays. It is not, however, uncommon to find some specimens with nine or even eight anal rays.

The type of *Notropis rubrifrons* is in the Philadelphia Academy of Sciences, where I had the pleasure of examining it a few years ago.

S. E. MEEK.

The Otocysts of Decapod Crustaceans. — An exhaustive study of the structure, development, and function of the otocysts of decapods

has been made by C. W. Prentiss.[1] In Palæmonetes, as in most other macrurans, the otocyst is a sac lodged in the basal segment of the antennule and opening dorsally by a constricted aperture partly covered by a scale-like fold. The sac is lined with cuticula which at the aperture is continuous with the animal's external shell. On an elevation rising from the floor of the sac is a horseshoe-shaped double row of from forty-five to fifty-eight hairs. The hairs are plumed and instead of being straight, as the tactile hairs of the outer surface are, they are bent so that the distal part of each shaft makes an angle of 120 degrees with its shorter base. Each hair is attached to the sac by a thin-walled chitinous bulb, thus allowing the hair as a whole to move freely. In the tangle formed by the crossing of the hairs are lodged fine grains of sand and organic detritus constituting an otolith. Every hair has at its base a group of matrix cells by which its chitinous wall was secreted. A single nerve fibre leads from the base of the hair inward to the brain, where it terminates in many fine branches. Each fibre has on its course a single cell body, so that each "auditory" hair is innervated by a single neurone. The same is true of the tactile hairs of the general surface, but the olfactory hairs, on the contrary, are innervated each by as many as a hundred neurones. Not only do olfactory and tactile hairs differ in this respect, but they can also be distinguished by the fact that in the olfactory hair the nerve fibres pass far out through the axis of the hair towards its tip, but in the tactile hair the single fibre ends at the base of the hair. An otocyst essentially similar to that in Palæmonetes was found in Crangon and in Cambarus.

In the common green crab, Carcinus, the otocyst is closed, contains no otolith, and its hairs are arranged in three groups instead of one. The innervation of these hairs is the same as in the macrurans studied.

Every time a shrimp or crab casts its shell, the cuticular lining of the otocyst, the attached hairs, and the otolith, if such be present, are discharged. As a preparatory step to this change, the matrix cells form a new hair under each old one, the new hair being half inverted in that the tip is pushed back into the base as the end of a finger of a glove might be infolded into the rest of the finger. When the skin is shed the new hairs are in part drawn out by the retreating skin to which they are slightly attached and in part

[1] Prentiss, C. W. The Otocyst of Decapod Crustacea: its Structure, Development, and Function, *Bull. Mus. Comp. Zoöl.*, vol. xxxvi (1901), pp. 167–251. 10 pls.

expanded by blood pressure. The discharge of the contents of the otocyst is through the natural aperture of the cyst, which remains open in macrurans but rapidly closes in brachyurans. Where an otolith is discharged, as in the shrimp and other macrurans, the animal immediately after ecdysis gathers in its claws small sand grains and puts them in the opening of the otocyst. These become attached to one another and to the auditory hairs by secretions from the walls of the otocyst and thus form a new otolith.

The otocyst was originally described as an organ of hearing. When sounds are produced under the water of an aquarium in which shrimps are contained, the animals respond by a darting movement if near the source of sound. The vibrations which stimulate the shrimps, however, can be *felt* by the submerged hand at a distance of ten to twenty centimeters greater than that at which the shrimps react. Moreover, shrimps respond to these vibrations even after the otocysts are removed. The reactions are inhibited, however, by the removal of the antennæ and antennules with their tactile hairs. It follows from these observations that whether we call the reactions auditory or tactile, the otocysts take little or no part in producing them.

If, then, the otocysts are not stimulated by sound, what is their function? When the otocysts are removed, shrimps swim with a more or less rolling motion and may even turn ventral side up. Their equilibration is thus shown to be seriously interfered with. When their eyes are covered with opaque materials so as to blind them, they swim with little or no rolling motion; but when both blinded and deprived of otocysts, they move with the greatest irregularity, swimming sometimes on their backs and sometimes in irregular spirals. Their capacity for orientation has disappeared completely, and as the experiments show, though the eye plays some part in keeping the animals upright, the otocyst is the chief sense organ in originating orientation reflexes. The otocyst is stimulated through the pressure of the otolith on the sensory hairs, as is shown by the ingenious experiments of Kreidl, who induced shrimp to form otoliths of iron particles instead of sand grains and then found that the animals became oriented to the lines of force of a magnet as they formerly did to gravity. These results are confirmed by Prentiss.

The development of the otocyst is also dealt with. It is not present in the first larval stage of the lobster nor is it more than indicated in the second and third, but at the fourth stage it suddenly appears with almost the full complexity of its adult structure. The locomotion of

the animals shows a corresponding change; young lobsters swim with great irregularity until the otocyst is developed, after which they regularly assume the upright position. It is also interesting to observe that immature lobsters newly molted and not allowed to form a new otolith reassume the rolling movements of their earlier stages and continue in these until opportunity is given them to form new otoliths. Thus the results obtained by Prentiss confirm completely the view first advanced by Delage, namely, that the otocyst has as its chief function that of originating orientation reflexes. That it is not an organ of hearing cannot with so much certainty be maintained, although its importance in this respect has assuredly been shown to be very slight.

P.

Recent Work on Anopheles. — In a recent study on the geographical distribution of Anopheles in relation to the former distribution of ague in England, G. H. F. Nuttall records with L. Cobbett and G. Strangeways (*Journal of Hygiene*, Vol. I, January, 1901) a series of observations of general interest in addition to much that is purely medical. He finds that three species, *Anopheles maculipennis*, *A. bifurcatus*, and *A. nigripes*, occur in Great Britain in all districts formerly malarious, but extend into regions in which no ague is known to have been prevalent at any time. To-day Anopheles is most numerous in low-lying land containing stagnant or slow-flowing water and corresponding to the districts where ague was formerly prevalent. As the disappearance of this disease does not depend upon the extinction of Anopheles, possible causes, therefore, are: (*a*) a reduction in the number of these insects consequent upon drainage of the land; (*b*) a reduction of the population in infected districts by emigration; (*c*) the use of quinine; or (*d*) the extinction of another yet unknown intermediary host besides man capable of harboring the parasite. The coincidence of the geographical distribution of ague and Anopheles is certainly not as precise as claimed by Grassi, and probably the *numerical* distribution will prove of equal importance. The presence of Anopheles in non-malarious districts explains the occasional occurrence of ague if a malarious subject comes in from other parts.

In another paper on the structure and biology of Anopheles, Nuttall and Shipley (*Journal of Hygiene*, Vol. I, January, 1901) give a full summary of our knowledge, together with personal observations. Noteworthy is the fact that the larva of Anopheles, as also that of Dixa, browse upon matter adhering to the surface film, like

certain fresh-water snails and many Turbellaria. Palmate hairs on the third to the seventh abdominal segments, with branches in the form of a cup, constitute the means by which the larva cling to the surface film. The studies are to be continued, and from their precise and detailed character bid fair to become a most valuable source of information regarding the genus.

H. B. W.

The Insect Book. — The complete title[1] indicates the effort of the author and of the publishers. The former is widely known for many valuable monographs, both systematic and biological, and for energetic and successful practical work; the publishers are equally well known for their enterprise in numerous undertakings tending towards the promotion of nature study. Such a combination should, it would seem, be eminently satisfactory. That it is not so is disappointing; that the title should be modified is evident. The *Insect Book* should not exclude the groups to which the "majority of collectors of insects confine their attention"; moreover, the standard must be low that considers the life histories "full"; the tables are confined to families or higher groups, and the bibliography, pp. 405–416, copied with but few additions from Banks's List (*Bulletin 24*, Division of Entomology, U. S. Department of Agriculture, 1900), however useful from a taxonomic standpoint, is not of primary importance in a book planned "to encourage the study of life histories of insects"; moreover, the very evident deficiencies of Banks's work are unnoticed and the mistakes not only are uncorrected but are augmented.

The text is readable, though not altogether well balanced in proportion; it shows evidence of haste, of a lack of continuity, and also of the most important essentials for a popular book, namely accuracy and conciseness. Dr. Howard evidently does not believe that all praise should be left for the tombstones, but his extravagant words for several contemporary authors can but jar many of his readers.

Some of the errors of statement are amusing, others are serious: under the former may be placed the classification (p. 295) of the book under review as one of the "older books" relating to insects;

[1] *The Insect Book.* A popular account of the bees, wasps, ants, grasshoppers, flies, and other North American insects, exclusive of the butterflies and moths and beetles, with full life histories, tables, and bibliographies. By Leland O. Howard, Ph.D., Chief of the Division of Entomology, U. S. Department of Agriculture. New York, Doubleday, Page & Co., 1901. xxvii + 429 pp., 47 pls., 265 text illustrations.

among those of a more serious nature may be mentioned the reproduction (pp. 343, 344) of Scudder's original notation attributing a day song and a night song to *Oecanthus niveus*. These supposed "day" and "night" songs have long been known to be the songs of distinct species, and so far back as 1893 Scudder recognized his error and stated that a revision of his score was desirable.

The plates are all original; most of the text-figures have been previously used and the source is acknowledged, though not in all cases satisfactorily.

Very few of the illustrations, either in the plates or in the text, can be commended; this can be clearly seen by comparing the colored plates with those of Holland's *Butterfly Book*, and the text-figures with those from the same blocks as used in various publications of the United States Department of Agriculture and elsewhere; in the make-up of the book the plates are poorly placed.

Inconsistencies in typography and lack of ordinary care in proof-reading are so apparent that even an inexpert corrector of the press could not turn many pages without finding glaring errors. S. H.

Michaelsen's Oligochæta.[1] — When Dr. Michaelsen began his work he had before him no easy task, and it is safe to say that no student of this class of animals could have discharged his duty in a more satisfactory manner. The museum in Hamburg is in possession of the largest, and as regards species most complete, collection of Oligochæta, and when we remember that it has been brought together entirely by Dr. Michaelsen, it may be readily understood that the author has been eminently fitted for his work. The literature of this class of animals is already very large, and it is also so scattered that it is impossible for any one living outside of the large literary and scientific centers of Europe to procure or have access to all. Dr. Michaelsen has had access to all this literature, and has personally examined almost every species described by different authors, as far as they yet exist, and the result has been a work of the very highest value, and one which no student of the group can do without.

The first part of the book contains a list of abbreviations for reference to literature. It seems to the reviewer that it would have been better if such abbreviations had been adopted as are used by the publications abbreviated. This suggestion refers equally to

[1] Michaelsen, W. *Das Tierreich*. 10. Lieferung, Vermes, Oligochæta. Berlin, Friedlaender, 1900. xxix + 575 pp., 13 figs.

almost every publication issued, and is a subject worthy of international consideration. Next after the abbreviations we come to a systematic index. When we consider that this index of families and species occupies not less than sixteen pages in double columns, we may begin to comprehend the enormous increase in described species during the last few years. All in all, there are probably 1100 species of Oligochæta now sufficiently well described to be readily identified. As is well known, the majority of species of this class can only be identified by anatomical characters. This makes identification difficult without the aid of illustrations, and it is much to be regretted that so few are found in this otherwise so valuable monograph. The morphological part is confined to ten pages, and it is here that the want of illustrations is especially felt. The author introduces a number of new words to indicate structures, such as tanilobic, zygolobic, prolobic, etc., according to the various and different encroachments of the prostomium on the anterior somite, etc. Many of these names have been derived from the Greek and Latin and are most happily found, and we can only regret that the author has not seen fit to revise also the general nomenclature to such an extent that students of other languages could readily understand what parts and organs are referred to. Why not use, for instance, "spermatheca" instead of "samentasche," etc.? The larger part of the book is devoted to a systematic description of the species, their genera and families. These descriptions are models of conciseness, including all the principal exterior and interior characters necessary to define the species. A large number of names have been changed, priority being given to the oldest ones, according to the rules adopted by the German society of naturalists. In this department the author has made very thorough studies, and as a consequence we meet with many novelties which will be referred to in their respective places. Every genus is preceded by a key for identifying the species. Many will object to the style of key adopted. In our opinion the most desirable key in all systematic works is the one which, besides facilitating the finding of the species, at the same time gives us an idea of the systematic arrangement and relationships. The system adopted by the author gives no idea of the affinity of the species, but simply facilitates finding them. In adopting the present style of key the systematic review of the genera and species certainly suffers. This is especially the case in large genera, as, for instance, Dichogaster (Benhamia, etc.), where the species number over 150.

The treatment of the limicolide groups should prove especially interesting to the American students, as the scattered literature of these groups is especially difficult of access, as much of it is in Slavic languages. We are glad to see that the genus Ilyodrilus has been restricted to its three California species and separated from Branchiura, with which genus it has really no affinity. The treatment of Enchytræidæ is especially good, the author having previously studied this family, and being the first one to bring order out of the chaos which existed previous to his monograph on that family. Among innovations we remark the genus Lumbricillus substituted for Pachydrilus. It is to be regretted that this change should have been necessary, as we have already too many names with a similar sound. The family which probably interests us most is the large one of Megascolecidæ. This family is made to contain the majority of North American terrestrial earthworms. The most interesting fact connected with the family as defined by the author is that it is made to contain Ocnerodrilus as well as Diplocardia, Trigaster, Plutellus, Chilota, etc. The genus Achantodrilus, which a year ago contained more species than almost any other genus, is now restricted to a single species. The Old-World species of the once large genus have now been separated from the New Zealand ones, while the majority of the species have been referred to the genus Notiodrilus, etc. It is most interesting to note that as a result of the author's investigations we now find Achantodrilus placed next to Microscolex and its allied genera, while a few years ago these were referred to different families. The genus Plutellus has been resurrected, as is quite proper, but what will astonish the general student more is that Plutellus and Pheretima have been referred to the same family, — the reason being that we now place very little importance on the number of setæ in each somite, as long as the interior organs resemble each other. The author has had special opportunity to examine Kinberg's types in the Stockholm Museum. As a result of this investigation it has been possible to identify nearly all of Kinberg's genera and most of his species, and consequently many later names have to give way to the old Kinberg names, — Pheretima instead of Perichæta, etc. The subfamily Diplocardinæ has been well treated, and so has its near relative Trigastrinæ. The Zapotecia has been raised to a full genus, it previously having been considered a subgenus only. Dichogaster is made to contain both Benhamia and Dichogaster, which certainly is an easy way out of the difficulty in defining the relationship of these two genera. But even the author's

lately established genus Balanta has had to give way to the all-engulfing Dichogaster. A division into subgenera retaining such names as Benhamia, Balanta, and Dichogaster would probably have been more in accordance to the ideas of several other investigators, and would have served to give a better review of this genus — we confess one of the most puzzling in the class. Under Ocnerodrilinæ we now meet with the genus Kerria, the structure of which is the one that gave the clue to the relationship of these genera with one and two pairs of prostates. Several of the subgenera under Ocnerodrilus have been raised to independent genera. We have thus Nematogenia, Pymeodrilus, and Ocnerodrilus side by side. *Nematogenia panamæsis* has been raised to an independent species. Among the subgenera Enicmodrilus has been fused with Ilyogenia. It would have been an improvement if the species followed each other in the general text in the same manner as in the key, or *vice versa:* this is rarely the case. The name Geoscolecidæ has been changed to Glossoscolecidæ, the latter being an older name.

The last family, Lumbricidæ, has more of a negative interest to students in this country, as comparatively few indigenous species are found here. While it is true that several species of this family have been described as new from North America, it is doubtful if they really are indigenous, and we may find that the species so considered are mere importations. This fact is certain, that no new Lumbricidæ have been found west of the Rocky Mountains. All the species on the Pacific coast are undoubtedly importations from Europe, and a very careful search by the reviewer has failed to reveal a single new species of this family between Alaska and Central America. This is the more interesting as the Pacific coast is in many respects related to Japan and eastern Asia. Thus Pillsbury has shown that the dart-bearing Helices are only found on the Pacific coast of North America, while they are widely distributed through the continents of Europe and Asia. Similarly among the Crustacea the genus Astacus is found all along the Pacific coast, and in Asia and Europe, while the genus Cambarus is found only in central and eastern North America. Among plants also there is a similarity between the Pacific coast and Japanese forms. But when we come to the earthworms the conditions are different. As far as we know, not a single species is found common to Japan or Asia and the Pacific coast. The indigenous California species seem all to be related to antarctic ones, or rather to species from south of the equator and to genera which with good reason may be considered as

having emanated from a former antarctic continent. The indigenous terrestrial species of Lumbricidæ found in North America should for the present be looked upon with suspicion in connection with their true habitat. The family of Lumbricidæ has received model treatment in Michaelsen's monograph. The book ends with a very perfect register, — a desideratum not always found in transatlantic scientific works. Taking it all in all, this splendid monograph cannot be too highly praised. It is not only indispensable to all students of the class, but it is so complete that for the identification of the species the older literature is made almost superfluous. As regards nomenclature, Dr. Michaelsen's ideas are almost final; while as regards classification they are sure to remain unchanged for many years. A perusal of the volume shows one thing which should be encouraging to all students of earthworms; that is, compared to the terrestrial Oligochæta the limicolide forms are almost unknown, and it is evident that it is among the latter that the greatest novelties will be found in the future.

GUSTAV EISEN.

BOTANY.

A Botany for Children. — Professor Atkinson in an attractive little book entitled *First Studies in Plant Life*[1] follows the current practice of presenting vital phenomena to the young beginner, rather than details of form. The first part gives a brief account of the growth and parts of plants, the second and third discuss their work and behavior, the fourth gives the life story of a sweet pea, an oak, a fern, a moss, and a mushroom, and the fifth treats of the battles of plants in the world. The illustrations are of exceptional excellence. Many ingenious and simple experiments are introduced. Much of the text seems likely to interest young people, and any teacher may gain valuable suggestions from the book.

A prevailing fault, however, is a more or less obvious "writing down" to the young reader, becoming at times mere sentimentality such as intelligent children resent. It is hard to see how the following passage, for example, can serve any useful purpose: "Some will

[1] Atkinson, George Francis, Ph.B., Professor of Botany, Cornell University. *First Studies of Plant Life*. Boston, Ginn & Company, 1901. 12mo. xii + 266 pp., 308 figs.

tell you that such interesting plants as the ferns, mosses, mushrooms, and puffballs are *cryptogams*, and that therefore you should not try to read the stories they have to tell. But why call them cryptogams? That is a terrible word, that ought to be blotted out of the English language. Why not call them plants, as they are? They are just as much God's creatures as the dandelion and thistle and smartweed are. They are just as interesting too, and mean as much in our lives as they do."

Furthermore, it may be questioned whether some of the topics to which considerable space is given — as, for example, turgidity, plasmolysis, and various microscopic details — are really within the comprehension of young beginners. Every one who has studied children knows what confused and perverted ideas they will often get regarding matters of much greater simplicity than belongs to some of the physiological topics here presented. It is hard enough to give college students clear ideas of microscopic mechanisms and life processes.

Only a few errors of statement have been noticed, but there is one which is sure to bewilder the pupil. On page 14 it is said of the bean that the embryo plant (meaning the plumule) is attached to the cotyledons. Then on page 19 it is asked if this small object which looks like a tiny plant is the embryo, and the reader is left to suppose that it is. Finally, on page 22 the reader is told most impressively that all inside the seed coat and its lining is the embryo, and that the embryo thus includes the cotyledons.

F. L. S.

A New Publication on Woody Plants. — Houghton, Mifflin & Co. announce that they will issue next autumn the first part of a new publication, *Trees and Shrubs*, consisting of text edited by Professor Sargent, and plates from drawing by Mr. Faxon, pertaining to woody plants, particularly those adapted to the gardens of Europe and the United States, or of commercial or economic importance. The sample pages and plates that have been distributed with the prospectus show, as would have been expected, excellence in drawing and publication, and the happy mean between technicality and popular writing which mark the *Silva* is likely to be maintained by Professor Sargent in this new publication, which in size and general appearance will bear considerable resemblance to the *Silva*. Two parts, each consisting of twenty-five plates and costing $5.00, are expected to appear yearly.

The prospectus of this proffered work, curiously enough, raises a question that may bother the botanical nomenclaturists, for Pl. II and the accompanying text depict and describe what is called a new species in that much-vexed genus Cretægus, so that when the initial number of the publication appears it will doubtless be found necessary to refer in it to this prior distribution of the species referred to.

T.

Flowers and Ferns in their Haunts.[1] — Where to draw the line between pleasant summer reading and didactic literature is often hard to decide, and yet when we go to the country there is a limit to what our trunks will carry, so that some kind of discrimination becomes necessary. This pretty little book will hardly come amiss, from whichever point of view selected, though it may not hold the attention of the reader like a novelette nor suit the needs of a class in botany; but through its pages runs a chatty narrative that is pleasing, and the illustrations show much that can be done by aid of the camera when intelligently used.

T.

The Dictionary of Gardening.[2] — As was stated in the *Naturalist* for November, 1900, the excellent *Dictionary of Gardening* of Mr. George Nicholson, curator of the famous Kew Gardens, which for years has been the reference book for gardeners wherever English is read, has had planned for it a supplement, bringing it up to the end of the century. The first volume of this supplement appeared in June, 1900, and a second volume, completing it, was distributed in July of the present year.

No more favorable place than Kew could be found for the elaboration of a compendious work on cultivated plants and the most successful ways of growing them. Not far from 25,000 species are said to be cultivated there. Kew is probably freer than any other establishment in the world from the common fault of botanic gardens, that the collections are grown uncritically under whatever names are attached to them when they are procured; and the very common, if often necessary, defect of botanical gardening, that a great variety of plants requiring dissimilar treatment are huddled together into a

[1] Wright, Mabel Osgood. *Flowers and Ferns in their Haunts*, with illustrations from photographs by the author and J. Horace McFarland. New York, The Macmillan Company, 1901. xix + 358 pp.

[2] Nicholson, George. The Century Supplement to the *Dictionary of Gardening*, a practical encyclopædia of horticulture for gardeners and botanists. George T. King, Hyde Park, Mass.

single house, where they collectively get the handling that best suits their average needs while it is not precisely adapted to any one species, is so far overcome at Kew that representatives of the great English plant houses sometimes go there for training. Mr. Nicholson is at once a good gardener and a lover and student of plants, and he has had the assistance of the best specialists at Kew and elsewhere, so that the *Dictionary*, as now completed, is a work alike valuable to the student of plants and their amateur and professional grower, and it cannot be spared from the shelves wherever plants are grown in variety. T.

Bailey's Botany.[1] — Carrying out his well-known ideas that botany in the secondary schools should begin with the commoner and grosser plants rather than by the use of those demanding the aid of the microscope, Professor Bailey has added another to the series of text-books already well and favorably known. Observation, experiment, and thought are thrust at the pupil throughout it, and the author very neatly acknowledges his obligation to hundreds of young people in many places for instruction "in the point of view," for the book, he tells us, is made for the pupil and, therefore, most appropriately views things as he sees them, even though it may enlarge his view of them before they are dropped. T.

Notes. — Under the guidance of Professors Flahault and Geddes, the late Robert Smith had begun the preparation of a series of botanical maps of Scotland, and since his death two sheets, respectively of Edinburgh and vicinity and northern Perthshire, have been issued in convenient pocket form, with a brief descriptive pamphlet, by John Bartholomew & Co. of Edinburgh.

W. N. Suksdorf has recently distributed excerpts from the *Deutsche botanische Monatsschrift*, extending over the period between November, 1898, and June, 1901, in which are published descriptions of a considerable species of Washington plants believed by him to be new to science.

Several new species and varieties of Californian plants are described by H. M. Hall in the *Botanical Gazette* for June.

Professor Dudley contributes an interesting and well-illustrated paper on the Big Basin Redwood Park to the *Forester* for July.

[1] Bailey, L. H. *Botany*. An elementary text for schools. New York, The Macmillan Company, 1900. xii + 356 pp., 500 figs.

A second part of Professor Nelson's "Contributions from the Rocky Mountain Herbarium," published in the *Botanical Gazette* for June, deals with various Colorado species and new western Arnicas and Eupatorieæ.

Four papers on the botany of the Yukon territory, as exemplified in the collections of R. S. Williams and J. B. Tarleton, are published in No. 6 of the current second volume of the *Bulletin of the New York Botanical Garden.*

The Botanical Seminar of the University of Nebraska has published the results of recent studies on the vegetation of that state.

A catalogue of the flora of Montreal Island, Canada, is being published in current numbers of the *Bulletin de l'Académie Internationale de Géographie Botanique.*

A systematic list of the plants collected by Schmidt on the Danish expedition to Siam in 1889 and 1900 is being published in the *Botanisk Tidsskrift.*

Pilger's "Beitrag zur Flora von Mattogrosso" is concluded in the second Heft of Bd. XXX of Engler's *Botanische Jahrbücher*, which, like other recent numbers, is also devoted in large part to studies of African plants.

Stanleya pinnatifida is figured in the *Gardeners' Chronicle* of June 15.

Dr. Small reviews the Mimosaceæ of the southeastern United States in the *Bulletin of the New York Botanical Garden* of May 27.

Rosa Engelmanni is figured in the April fascicle of *Icones Selectæ Horti Thenensis.*

Two species of Epilobium are characterized by Suksdorf in the *West American Scientist* for May.

An interesting "Study of the Papaw," *Carica Papaya*, by F. B. Kilmer, has run through several recent numbers of the *American Journal of Pharmacy.*

The Canada thistle is the subject of a revised issue of *Circular 27 of the Division of Botany of the United States Department of Agriculture*, by Mr. Dewey.

Several new Canadian gentians are described by Holm in the *Ottawa Naturalist* for July.

In the *Bulletin of the Torrey Botanical Club* for June, Dr. Small proposes the generic name Brayodendron for what has been known as *Diospyros Texana*, and describes several species in the genera Quercus, Æsculus, Hypericum, Azalea, and Dendrium.

Dr. Rydberg discusses the oaks of the continental divide north of Mexico in No. 6 of the current volume of the *Bulletin of the New York Botanical Garden.*

Vol. I of Kraenzlin's *Orchidacearum Genera et Species*, devoted to the groups Apostasieæ, Cypripedieæ, and Ophrydeæ, is completed with the 16th fascicle, the preface of which bears the date May, 1901. The volume, though its titlepage bears the date 1901, has been in publication since 1897.

An important article on the Texan home of *Pinus Cubensis*, by Professor Bray, appears in the *Forester* for June.

The report of the society *Isis* of Dresden for 1900 contains Parts I and II of a paper by Menzel on the gymnosperms of the North-Bohemian lignite formations, illustrated by a number of plates.

To the rather numerous check lists of ferns and fern allies is added another by B. D. Gilbert, bearing the imprint of L. C. Childs & Son, Utica, N. Y., 1901. Unlike most such lists, this one, which includes 438 numbers (species and varieties) which occur in North America above the Mexican line, contains a considerable number of critical notes and descriptions of new species.

A study of the nectar glands of *Pteridium aquilinum*, by Professor Lloyd, is published in *Science* for June 7.

A monograph of North American Sordariaceæ, by David Griffiths, constitutes the opening number of Vol. XI of the *Memoirs of the Torrey Botanical Club.* It is illustrated by 19 plates and several figures in the text, and contains a bibliography and index.

Part III of Arthur and Holway's "Descriptions of American Uredineæ," in No. 2 of the current volume of the *Bulletin from the Laboratories of Natural History of the State University of Iowa*, deals with rusts of several groups of grasses.

From a short paper by Professor Burt, published in the *Bulletin of the Torrey Botanical Club* for May, *Tremella mycetophila* Pk. appears referable to the genus Exobasidium.

Professor Atkinson, in *Bulletin 193 of the Cornell University Experiment Station*, gives an account of several of the fungi which attack the wood of shade and timber trees, — a subject heretofore investigated in this country by Dudley and von Schrenk.

The Manchester Literary and Philosophical Society, in a recent number of its *Memoirs and Proceedings*, prints an interesting lecture on the flora of the human body by Dr. Metchnikoff.

A doctor's thesis by S. L. Schouten, on the methods of securing pure cultures of micro-organisms from a single cell isolated under the microscope, has been issued from the University of Utrecht.

The present knowledge of the causation of certain plant diseases by bacteria is well brought out in a series of controversial articles by Professor Fischer and Dr. E. F. Smith, in the *Centralblatt für Bakteriologie und Parasitenkunde*, extending over the last two or three years, separates of which have recently been distributed by Dr. Smith.

Ule shows, in Heft 2 of the current volume of Engler's *Botanische Jahrbücher*, that a number of Amazonian Epiphytes seem to owe their distribution in part to certain ants which take to their nests in trees seeds of the other plants, some of which vegetate there.

The initial number of what promises to be an interesting series of articles on the pollination of Chilian flowers is published by Dr. Jokow in Heft 3–4, Bd. IV, of the *Verhandlungen des deutschen wissenschaftlichen Vereins zu Santiago de Chile.*

Prometheus, of June 12, contains the first of a series of articles by Sajó on plums and other fruits of American origin.

Mr. Pinchot has issued as *Bulletin No. 30 of the Division of Forestry of the United States Department of Agriculture* a very practical forest-working plan for a portion of the New York forest preserve, prepared by Messrs. Hosmer, Bruce, and Newell.

The Imperial Botanic Garden of St. Petersburg feels the need experienced by most active scientific establishments of a medium of publication under its own control as to time and other important conditions, and the director of the garden, Professor A. Fischer de Waldheim, announces the early appearance of the first number of a *Bulletin* which the Garden is to issue.

The *Plant World* for June contains several interesting short articles, among them one on the Cuban uses of Oreodoxa, one on Bermuda, and some suggestions as to the points to be observed in collecting specimens of Cratægus.

A portrait and appreciative little sketch of Thomas Meehan appear in the *Gardeners' Chronicle* of June 15.

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(Regular exchanges are not included.)

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